

# SUPPLEMENTO

AL VOLUME XXII, SERIE X, DEL

# NUOVO CIMENTO

A CURA DELLA SOCIETÀ ITALIANA DI FISICA

1961

4° Trimestre

N. 1

## I N D I C E

R. N. THOMAS — Preface . . . . .	pag. vii
G. POLVANI — Opening address . . . . .	» xii
K. O. KIEPENHEUER — Schematic structure of the quiet sun . . . . .	» xiv
C. DE JAGER — Model of an active region of the sun . . . . .	» xiv
K. O. KIEPENHEUER and E. N. PARKER — Velocities and energies of active sun . . . . .	» xv
Photographic group of the participants . . . . .	» plate

**PART I. — Questions of general background and methodology relating to aerodynamic phenomena in stellar atmospheres.**

J.-C. PECKER and R. N. THOMAS — Summary-introduction . . . . .	» 1
Discussion . . . . .	» 44

**PART II. — General summary of results on «astronomical turbulence» in stellar atmospheres.**

A. B. UNDERHILL — Summary-introduction . . . . .	» 69
Discussion . . . . .	» 100

**PART III. — Spherically-symmetric motions in stellar atmospheres.**

**A. Pulsating variable stars.**

P. LEDOUX and C. A. WHITNEY — Summary-introduction: Velocity fields and associated thermodynamic variations in the external layers of intrinsic variable stars . . . . .	» 131
Discussion . . . . .	» 194

**B. The propagation of a shock-wave in an atmosphere of varying density.**

E. SCHATZMAN — Summary-introduction . . . . .	» 209
Discussion . . . . .	» 227

### C. Non-catastrophic mass-loss from stars.

A. DEUTSCH - Summary-introduction . . . . .	»	238
Discussion . . . . .	»	260
Re-discussion, from the viewpoint of aerodynamics.		
P. GERMAIN - Remarks on steady perfect fluid with spherical symmetry . . . . .	»	296
Discussion . . . . .	»	301

## PART IV. - Considerations on localized velocity fields in stellar atmospheres: Prototype - the solar atmosphere.

### A. Convection and granulation: Preview on granulation - Observational studies.

J. RÖSCH - Observations from the Pic du Midi . . . . .	pag.	313
E. SPIEGEL - The Princeton balloon observations . . . . .	»	319
R. B. LEIGHTON . . . . .	»	321
Discussion . . . . .	»	325
A. B. SEVERNY - The motions and magnetic fields in the undisturbed solar atmosphere (outside active regions) . . . . .	»	327
E. BÖHM-VITENSE - Summary-introduction.		
A) Observations . . . . .	»	330
B) Theory of the hydrogen convection zone . . . . .	»	338
Discussion . . . . .	»	346

### B. Considerations of convective instability from the viewpoint of physics.

W. V. R. MALKUS - Summary-introduction: Similarity arguments for fully developed turbulence . . . . .	»	376
Discussion . . . . .	»	385

### C. Transient velocity fields in the lower solar atmosphere.

A. B. SEVERNY - Summary-introduction . . . . .	»	403
Discussion . . . . .	»	416

### D. Collision-free shock-waves.

H. E. PETSCHEK - Summary-introduction: Collision-free plasmas . . . . .	»	448
A. A. BLANK and H. GRAD - Second summary-introduction: Steady one-dimensional fluid-magnetic collisionless shock theory . . . . .	»	459
Discussion . . . . .	»	468

## PART V. - Summaries.

H. LIEPMANN . . . . .	»	487
R. N. THOMAS . . . . .	»	494
R. LÜST . . . . .	»	500
Discussion . . . . .	»	505
M. MINNAERT - Closing remarks to the meeting . . . . .	»	512



## Preface.

R. N. THOMAS

*Boulder Laboratories, U. S. National Bureau of Standards.*

### 1. - Background of the symposium.

The present symposium, fourth in the series of symposia on Cosmical Gas Dynamics sponsored jointly by the International Union of Theoretical and Applied Mechanics and the International Astronomical Union, actually represents the blending of two lines of attempt to focus attention on aerodynamic phenomena in stellar atmospheres. One is the evolution of the present series of symposia itself. This series grew out of collaborative efforts by J. M. BURGERS and J. H. OORT to study the properties of interstellar gas clouds, and their feeling that collaboration between aerodynamicist and astrophysicist might be extended to other problems of the interstellar medium and galactic structure, where large differential velocity fields are observed. The stimulating effect of three such symposia on the interstellar medium led to the suggestion that the aerodynamical-astrophysical collaboration be extended to other areas of astrophysics — such as the stellar atmosphere — where observed velocity fields suggested interest in aerodynamic problems. The second line of interest grew directly among astronomers interested in the structure of stellar atmospheres, under the dual impetus of an enormous increase in detailed observational data and of an associated refinement in interpretative theory. An informal gathering of a few astronomers at the 1952 IAU meetings in Rome considered the question of whether a more careful examination of the concept of « astronomical turbulence » might not be useful. The discussion led to plans for a more extended symposium, to last several days, at the time of the Dublin IAU in 1955; and for preparation of an extensive bibliography covering astronomical papers on aerodynamic phenomena in stellar atmospheres. These plans were considerably condensed: no bibliography was prepared, and a 2.5 hour session was held at Dublin (*Trans. IAU* 9, 727 (1955)), the curtailment apparently reflecting the attitude of most astronomers that the subject was not at that time of sufficiently wide-spread



interest to justify such extensive attention. Thus the present symposium may be viewed as the culmination of these earlier efforts, from the standpoint of astrophysical interest in a more complete view of the structure of stellar atmospheres; as well as an extension of the collaborative efforts between aerodynamicists and astrophysicists in the general series of symposia of which this is the fourth.

The preface to the Proceedings of the Third Symposium (*Rev. Mod. Phys.*, **30**, 905 (1958)) contains an historical-conceptual summary of the evolution of astronomical concern with aerodynamical problems and of the introduction of the concept of «astronomical turbulence». This summary serves as well for this symposium on the stellar atmosphere as it did for that on the interstellar medium, and there is little point in repeating it here. It is however useful to stress two points made in that summary. One is that historically most astronomical concern with velocity fields in stellar atmospheres has centered on phenomena concerned with stellar pulsation and on the various measurements of «astronomical turbulence» — a term loosely used to denote random velocity fields in the stellar atmosphere. The second point is that an astronomical concern with aerodynamical phenomena, rather than simply with velocity fields, came when astronomers began to ask the mechanical energy dissipation likely to be associated with these velocity fields, rather than simply their effect on the radiative absorption coefficient or on the momentum balance in the stellar atmosphere.

For a number of years in the postwar era, astronomers have debated whether demands for a more realistic appraisal of the relation between «astronomical turbulence» and aerodynamical concepts represented a point of semantics or one of physics. In a similar way, it has been a controversial question whether mechanical energy dissipation effects were sufficiently large to have appreciable influence on models and concepts used in analysing stellar atmospheres anywhere except in extremely tenuous regions such as gaseous nebulae and the solar corona. In the past few years, it appears that astronomical opinion has shifted sufficiently to include even the denser regions of the solar chromosphere within the category of being subject to appreciable influence by these mechanical dissipative effects — raising the densities admitted for such a category from some  $10^8$  to some  $(10^{15} \div 10^{16})$  atoms/cm<sup>3</sup>.

This dual astronomical impetus arising from increase in detailed observational data and refinement in theory has stimulated concern with a third aspect of the velocity field-aerodynamic phenomena association, that of a more detailed interest in specific solar features. From one side, questions on the origin of the velocity field providing the mechanical dissipative effects in the solar chromosphere-corona led to more detailed interest in the observational properties and theoretical consequences to be associated with the observed solar granulation, and to investigations of the aerodynamic significance of



the observed solar spicules. From another side, extreme high-dispersion spectra of the solar surface (up to some  $20 \text{ mm}/\text{\AA}$ ) give strong evidence for small-scale, transient velocity features existing there.

Because this was the first symposium on aerodynamical phenomena in stellar atmospheres, and the possible breadth of the subject is so great, the program was planned to emphasize those points standing out in the manner just summarized. As a general preparation of background material for the Symposium, a number of astronomers collaborated in preparing a bibliography covering papers on aerodynamic phenomena in stellar atmospheres. In practice, the epoch that it seemed useful to cover went back to roughly 1920. A request was made by the aerodynamicist members of the Organizing Committee for some kind of a background-summary of astrophysical concepts and jargon, in the field covered by the Symposium. This was taken as an opportunity not only to present a background-summary from the standpoint of the conventional astrophysical approach, but also to look at the astrophysical methodology from a somewhat more critical standpoint, asking what effects could be expected if indeed aerodynamic dissipation effects had significant influence on the state of the stellar atmosphere. While such a discussion thus became strongly involved in the current controversy on the importance of including effects of aerodynamic dissipation, it was not thought useful to schedule as part of the sessions a discussion of the solar chromospheric studies referred to above. It was felt these were too specialized for this first Symposium.

The two topics already referred to as most studied in the astronomical literature were made the basis for the first two sessions on astronomical material proper: «astronomical turbulence» and pulsation phenomena. The scope of the latter session was widened, to include the subject of non-catastrophic mass-loss, to one of whose possible phases we must be led in studying the outer boundary conditions for the pulsation problem. Also a session was reserved under this heading for a more direct inquiry into problems arising from a shock propagating into a medium of decreasing density.

Attention was then directed at the more detailed studies of «localized» velocity fields that are possible by restricting attention to the sun. Considerable emphasis was placed on the solar granulation, and also an attempt was made to stimulate discussion, from the aerodynamicist's viewpoint, of the general problem of convective instability as it might be reflected by the granulation. It is hard to cover the wide range of material bearing on velocity fields in the solar atmosphere, and their possible association with other phenomena such as magnetic fields, as part of such broad sessions as this, so only a small sampling was included. There seemed to be a general feeling that one of the future symposia in the series might be confined to solar phenomena.



Finally, because of the general interest in energy dissipation mechanisms in those rarified atmospheres where magnetic fields occur, a discussion of collision-free shocks was included.

## 2. - Mechanics of the symposium.

The Fourth Symposium was held as one of the sessions of the 1960 International School of Physics «E. Fermi», under the auspices of the Società Italiana di Fisica, at the Villa Monastero, Varenna (Lake of Como), 18-30 August.

Like the three preceeding Symposia, it was organized by the International Union of Theoretical and Applied Mechanics and the International Astronomical Union. The Organizing Committee consisted of: IUTAM: BURGERS, LIEPMANN and SEDOV; IAU: MINNAERT, RIGHINI, SEVERNY, UNSÖLD and THOMAS. MINNAERT served as president; LIEPMANN and THOMAS as the secretariat.

Arrangements at Varenna were prepared by RIGHINI. We are indebted to the staff of the Villa Monastero, the Mayor and City Council of Varenna, and to the Italian Physical Society for their role as hosts to the Conference.

The two International Unions, with the assistance of UNESCO, made available limited funds for the partial defrayment of traveling expenses of some of the participants. The Consiglio Nazionale delle Ricerche di Italia generously made available a grant to aid in meeting the secretarial and other expenses at Varenna. The US National Bureau of Standards aided in the secretarial problem on obtaining transcriptions of the meetings. To meet an unexpected crisis in the transcribing equipment, the U.S. Air Force kindly provided equipment and personel, Mr. F. SLATER, and Mr. D. TADDE. G. COLCHAGOFF and S. CELLERAI organized the obtaining and initial transcription, of the records: to Mr. SLATER and Mr. CELLERAI, we are particularly indebted for the functioning of the recording equipment under trying conditions of severe power failures occasioned by storms.

The detailed prior arrangements from the standpoint of the organizing secretariat, the transcription of records in Varenna, and the subsequent compilation and preparation of corrected transcriptions and manuscripts have been handled by Miss ANNE TAYLOR of the NBS staff; at Varenna she has been aided by Miss NANCY POTTER of NBS.

Arrangements for publication have been made by G. POLVANI, President of the Italian Physical Society and by G. RIGHINI. The Society has generously supported the publication of these Preceedings as a supplement to *Nuovo Cimento*. RIGHINI, LIEPMANN and COLCHAGOFF have, together with THOMAS, acted as an Editorial Committee on the Proceedings. Rough transcripts of the several sessions were provided to the participants for correction as they



were transcribed. These formed the basis from which an edited version of the proceedings has been prepared. Considerable condensation and re-ordering of material has been made in the editing process. Because the main use of such proceedings as these lies in the degree to which they summarize current thinking, the major concern has been to ensure their prompt publication. Because of this, no possibility has been given the participants to check the edited version; responsibility for errors and misconceptions thus fall on THOMAS, who offers apologies for these wherever they may occur.

The list of participants is as follows:

*Aerodynamicists and Physicists.* — C. AGOSTINELLI, Italy; G. K. BATCHELOR, England; A. A. BLANK, USA; G. F. CARRIER, USA; F. H. CLAUSER, USA; G. COLCHAGOFF, USA; A. CRAYA, France; L. DAVIS, USA; P. GERMAIN, France; S. GOLDSTEIN, USA; M. KROOK, USA; R. B. LEIGHTON, USA; H. LIEPMANN, USA; S. LUNDQUIST, Sweden; W. V. R. MALKUS, USA; N. MILFORD, USA; H. PETSCHKE, USA; V. S. SAFRANOV, USSR; L. SCHIFF, USA; V. D. SHAFRANOV, USSR; W. B. THOMPSON, England; M. S. UBEROI, USA.

*Astronomers.* — L. BIERMANN, Germany; K. H. BÖHM, Germany; E. BÖHM-VITENSE, Germany; A. BRUZEK, Germany; E. M. BURBIDGE, USA; G. BURBIDGE, USA; I. K. CSADA, Hungary; C. DE JAGER, Netherlands; A. J. DEUTSCH, USA; G. ELSTE, Germany; E. G. FORBES, Scotland; G. GODOLI, Italy; M. HACK, Italy; S. S. HUANG, USA; F. KAHN, England; K. O. KIEPENHEUER, Germany; P. LEDOUX, Belgium; R. LÜST, Germany; W. H. MCCREA, England; M. MINNAERT, Netherlands; E. A. MÜLLER, USA; A. G. PACHOLCZYK, Poland; B. E. J. PAGEL, England; E. N. PARKER, USA; C. W. PECKER, France; J.-C. PECKER, France; S. POTTASCH, USA; K. H. PRENDERGAST, USA; H. V. REGEMORTER, France; G. RIGHINI, Italy; J. RÖSCH, France; E. SCHATZMAN, France; M. J. SEATON, England; A. B. SEVERNY, USSR; E. SPIEGEL, USA; ZD. SVESTKA, Czechoslovakia; R. N. THOMAS, USA; J. TUOMINEN, Finland; A. UNDERHILL, Canada; A. UNSÖLD, Germany; J. WADDELL, USA; C. A. WHITNEY, USA.

## Opening Address

G. POLVANI

*President of the Italian Physical Society.*

Yesterday, Prof. RIGHINI invited me to say some introductory words at this opening ceremony for the Symposium on Aerodynamics in Stellar Atmospheres.

At first, I really wanted to refuse this task; but now, on the contrary I am very glad to have the opportunity to answer to his many kind remarks. Therefore, I would like to thank him for all this, also in the name of the Ente Villa Monastero.

Prof. RIGHINI has been here twice previously, as Director of summer courses of the Società Italiana di Fisica which dealt with topics in some way connected to those of this symposium. This return to Varenna, promoted by Prof. MINNAERT and by many others last year, shows how much everybody has become fond of this place.

On behalf of the Ente Villa Monastero and of the Società Italiana di Fisica, I personally am delighted to say « welcome », or more precisely « bentornati », to Prof. RIGHINI, to Prof. MINNAERT and to all the participants of this symposium. I sincerely and fervently hope that you all will once again return to this wonderful spot on the Lake of Como, in Italy.

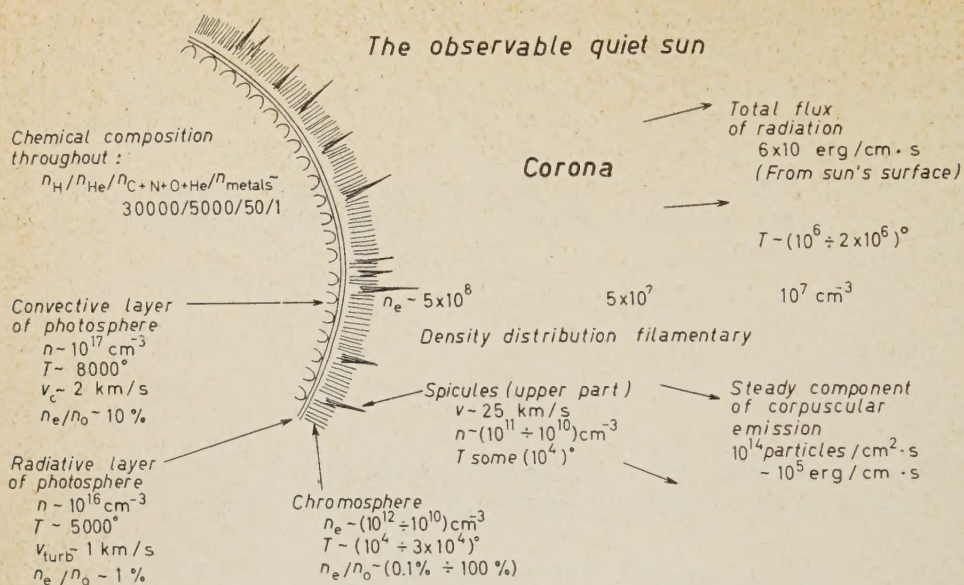
I hope you will excuse me for this topographical crescendo, for this geographical amplification of my speech: I must confess that I love Varenna and the people of Varenna, so rightly proud of their history, which is full of important events, but to speak of them is not the purpose of my speech.

I would rather put a question to Prof. RIGHINI, who dwells in that part of the world that I also consider as my region: Tuscany. The question refers to one of those centenary or semi-centenary celebrations which generally take place in remembrance of an important event or person. Do you remember what happened 350 years ago, precisely on July 1610? One of the greatest discoveries, I would even say the greatest and most peculiar discovery relating to aerodynamics of stellar atmospheres — Galileo's discovery of sunspots. This symposium is the most proper and highest celebration of this discovery



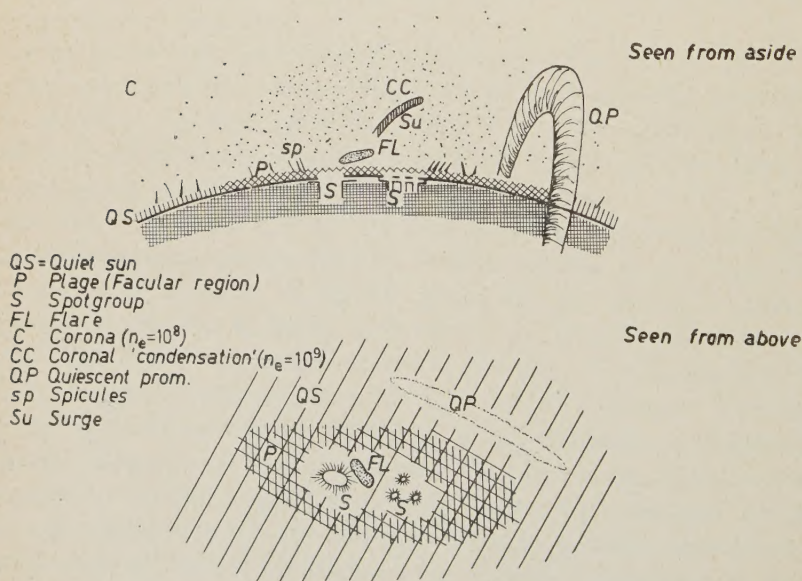
that we could imagine. And it is of great interest to me that nobody, not even the astronomers, have thought of this coincidence. So I am grateful to all of you, especially to Dr. RIGHINI, for this oversight — so that I might propose to the organizers of this symposium that it be dedicated spiritually to Galileo. Is it too high an expectation for the outcome of this symposium at Varenna, that I anticipate? We shall see. I propose to the Council of the Society that they publish the Proceedings of this Symposium in their journal, *Nuovo Cimento*.

# Schematic Structure of the Quiet Sun



K. O. KIEPENHEUER

## Model of an Active Region of the Sun



C. DE JAGER



# Velocities and Energies of Active Sun.

TABLE I. - *Velocities.*

Duration	Phenomena	Velocities	Notes
Weeks	Plage regions	$\sim (0.5 \div 2)$ km/s	Doppler
Days	Sunspots	$(0.5 \div 8)$ km/s	Doppler, away from center of disk
Months	Quiescent prominences	Internal motions 10 km/s	Doppler and visible displacement
Hours	Ascending prominences	$(50 \div 700)$ km/s	Displacement
Hours	Coronal motion (internal)	10 km/s	
Minutes	Coronal whip	$\sim 600$ km/s	
Minutes	Flares	Internal motions $(0 \div 600)$ km/s	Doppler
Minutes	Flare surges	$(50 \div 250)$ km/s	Doppler and visible displacement
Minutes	Steady streams and flows of gas producing sequences of terrestrial disturbances	$\leq 1000$ km/s	
Minutes	Effect of flares on existing prominences	$(100 \div 1000)$ km/s	
Minutes	Effect of flares on triggering other flares	$(1000 \div 1500)$ km/s	
Minutes	Radio bursts type II (flare-associated)	$\sim 1000$ km/s	
Seconds	Radio bursts type III (flare-associated)	$\sim \frac{1}{3} c \div \frac{2}{3} c$	
Hours	Radio bursts type IV (flare-associated)	$\sim 500$ km/s	Highly correlated with ensuing geomagnetic storms

TABLE II. - *Energies.*

Large flare:	$> 10^{32}$ erg (radiated energy) $> 10^{30}$ erg (particle emission, 10 MeV $\div$ 30 GeV per proton)
Radio emission	from large flare $\sim 10^{25}$ erg Ejected mass $\sim 10^{19}$ g (total $\sim 10^{34}$ erg) Total energy content of quiet corona $\geq 10^{32}$ erg Implies annihilation of 500 G in the flare

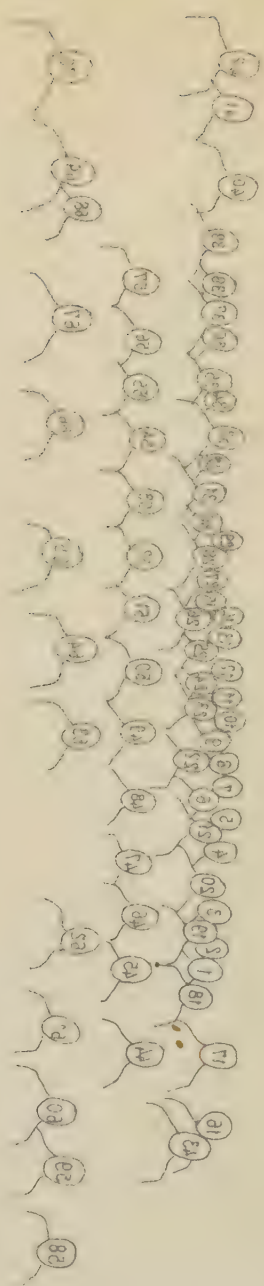
Prepared by K. O. KIEPENHEUER and E. N. PARKER.







1. P. Ledoux
2. K. H. Prendergast
3. Z. Svstka
4. J. Waddell
5. B. E. J. Pagel
6. C. Agostinelli
7. N. Milford
8. C. de Jager
9. E. G. Forbes
10. M. Krook
11. I. K. Csada
12. A. G. Unsöld
13. A. G. Pacholczyk
14. G. Colchagoff
15. A. B. Severyny
16. E. Spiegel
17. C. A. Whitney
18. H. Liepmann
19. K. O. Kiepenheuer
20. J. Rösch
21. H. v. Regemortier
22. J. C. Pecker
23. S. Pottasch
24. V. S. Safranov
25. G. Godoli
26. Huang Su-Shu
27. R. N. Thomas
28. W. H. McCrea
29. H. K. Böhm
30. F. Kahn
31. W. V. R. Malkus
32. G. F. Carrier
33. S. Lundquist
34. L. Biermann
35. M. J. Seaton
36. G. Burbidge
37. L. Davis jr.
38. N. Parker
39. E. M. Minnert
40. M. Germain
41. P. A. Deutsch
42. A. J. Deutsch
43. V. D. Shafranov
44. R. B. Leighton
45. Mrs. Craya
46. Mrs. Röscht
47. Mrs. Ledoux
48. Mrs. Pottasch
49. E. Böhm-Vitense
50. Mrs. Ledoux
51. A. B. Underhill
52. E. M. Burbidge
53. C. W. Pecker
54. F. A. Müller
55. Mrs. Elste
56. Mrs. Elste
57. Mrs. Lüst
58. S. Cellier
59. H. E. Peischek
60. A. Bruzek
61. A. Taylor (Secretary)
62. N. Potter (Secretary)
63. Secretary
64. J. Tuominen
65. C. Guesh
66. E. Schatzman
67. G. Elst
68. Miss Clauser
69. F. H. Clauser
70. G. Righini

[illegible][illegible][illegible][illegible]



FOURTH SYMPOSIUM ON COSMICAL GAS DYNAMICS  
VARENNA ON LAKE COMO - VILLA MONASTERO - August 18-30, 1960







## PART I.

# Questions of General Background and Methodology Relating to Aerodynamic Phenomena in Stellar Atmospheres.

### Summary-Introduction

J.-C. PECKER and R. N. THOMAS

*Observatoire de Meudon and Boulder Laboratories-NBS*

#### 1. - Introduction.

This paper is an introduction to the astronomical material underlying the Varenna Symposium on Aerodynamical Phenomena in Stellar Atmospheres. The term « aerodynamical phenomena » rather than simply « velocity fields » is used in the title of the symposium to imply that primary concern centers as much on the physical phenomena and consequences associated with the presence of velocity fields as it does simply on the velocity fields themselves. To fully appreciate this distinction between aerodynamical phenomena and velocity fields from the astronomer's viewpoint, one must consider it against the background of the *classical theory* (\*) of stellar atmospheres, which assumes that all the properties of the atmosphere are strictly controlled by the radiation field. The thermodynamic state of the classical atmosphere is fixed by the three conditions of radiative equilibrium (no energy transport other than by radiation), hydrostatic equilibrium (no mechanical momentum transport), and local thermodynamic equilibrium at a temperature fixed by the local energy-density of the radiation field (complete coupling between radiation field and atomic degrees of freedom). Analyses of stellar spectra under the framework of this classical atmospheric model take account of the presence of velocity fields (other than thermal) only in their effect upon the atomic

---

(\*) It is necessary to distinguish between what astronomers call *empirical* models of a stellar atmosphere and *theoretical* models, when discussing models of stellar atmospheres and the assumptions underlying their construction. An empirical model has as its basis an empirical determination of the distribution of  $T_e$  through the atmosphere. (There are, of course, assumptions underlying the particular empirical determination of  $T_e$ , which also may be questioned, as we discuss in the following.) The theoretical model is based wholly on a set of assumptions which suffice to determine the temperature structure. In the following, we hold literally to these definitions.

absorption coefficient, not in their energetic or momentum coupling to the thermodynamic state of the atmosphere. Thus, if we become interested in aerodynamic phenomena in stellar atmospheres, we must investigate the possible perturbation these velocity fields may have upon the thermodynamic state of the atmosphere. We develop a primary concern with differential motions, velocity gradients, and dissipation mechanisms — all quantities which may produce a local non-relative energy source — rather than directing our attention only at stellar rotation and uniform expansion of an atmosphere. Thus, what we call aerodynamic phenomena embraces not only velocity fields but also their influence upon the thermodynamic state of the atmosphere.

Granted such a primary concern with aerodynamic aspects, rather than simply with velocity fields as such, one must still recognize that the only *direct* observational approach to the existence of these aerodynamical aspects lies in empirical studies of velocity fields. The inadequacy of astrophysical exploitation of such empirical studies lies, for the most part, in failure to ask the physical consequences attending the existence of such observed velocity fields. The physical consequences are of two types, returning to the distinction between empirical and theoretical atmospheric models made earlier. First, on the side of empirical models, taking both velocity fields and thermal structure as known empirically, can we construct a self-consistent atmospheric model? Second, on the side of the theoretical models, taking the velocity fields as known empirically, what can we say about the modification of the assumptions, introduced to construct a theoretical model atmosphere and temperature distribution, over those assumptions used in the absence of the velocity fields? The whole class of mechanical energy dissipative problems, and their coupling with thermal structure, enters here.

These last considerations lead to the possibility of an *indirect* observational approach to the existence of aerodynamic phenomena, which would act to modify the thermodynamic state of the atmosphere over that predicted by the classical model, by looking for phenomena that would be anomalous under the classical model predictions.

The whole class of variable stars forms an immediate example, but there a primary source of information is observation of the velocity fields often associated with such variation. Magnetic and spectral variability suggest, but do not directly establish, the presence of aerodynamic (including hydro-magnetic) effects. Evidence for mass-ejection, usually but not always based on associated velocity measures, is another example. Possibly the most interesting kind of indirect evidence — from the standpoint of current interest in high-energy aerodynamics — is that based upon spectroscopic evidence for the existence of effects of non-equilibrium thermodynamics in the stellar atmospheres, of the kind that would be associated with a non-radiative energy supply perturbing the radiative control upon which the classical model is



based. The most detailed investigations of such evidence have thus far been carried out on the solar chromosphere and corona, and they are summarized in detail by THOMAS and ATHAY (1961). A number of the phenomena characterizing wide classes of stars have not been subjected to detailed analysis, but offer promise of providing the same kind of indirect evidence on aerodynamic phenomena. Examples are the presence of emission cores in the absorption lines of  $\text{Ca}^+$ ; and the presence of emission lines themselves in certain cases.

In the following, we limit ourselves to a summary of the methodology by which direct astrophysical knowledge of velocity fields in stellar atmospheres has been obtained. We stress both the conceptual basis upon which the methodology in current use rests, and the conceptual problems which have either been set aside for simplicity or have been ignored. These latter may raise non-trivial question on the interpretation of the results of existing analyses. We neither present nor discuss particular observational results, nor their interpretation; these points are covered in the individual summary-introductory papers of the Varenna program. We do not include here a discussion of the kind of non-equilibrium-thermodynamic effects upon which *indirect* inference on aerodynamic phenomena rests. Our aim is simply to present for the aerodynamicist-physicist participants some background in the conceptual basis upon which rest the astronomical inference of velocity fields; and to raise for the astrophysicist a critical commentary upon these matters.

We distinguish four kinds of inferential procedures upon which astrophysical knowledge of velocity fields is based: 1) and 2) refer to the empirical approach defined earlier; 4) to the theoretical; and 3) to something intermediate.

1) *Direct observation of motion at some angle to the line of sight.* — Such observations are possible only in a few cases. One is in the case of solar phenomena observed on the solar limb, such as prominences. Another is the case of shells in novae and supernovae. The primary problem in interpretation lies in separating the motion of an excitation front from the actual motion of material. The spectroscopic aspects of this problem are similar to those in 2) below. Aside from this complication, however, there is nothing particularly novel to the astronomical, as contrasted to any other, situation. There is no particular question of general methodology; so we do not consider this procedure further.

2) *Interpretation of a spectrum to infer line-of-sight motion.* — Such interpretation requires a detailed theory of the formation of a spectral line in a gaseous atmosphere, with and without the presence of velocity fields. Two extreme situations are particularly easy to interpret — one, a line symmetrically broadened by random motions in an optically thin gas; the

other, displacement of the line as a whole due to uniform motion of the entire body of gas. In general, however, the observed line profile is a composite of random and non-random motions which both displace and broaden the line in a complicated manner. We concentrate our discussion in this paper upon this alternative 2), from which comes by far the most of the astrophysical information.

3) *Partly-empirical, partly-theoretical inference.* — One observes some phenomenon, and from it infers the existence of some velocity field. For example, one observes the differential rotation of a star (the sun), and infers from it as a necessary physical consequence the existence of vertical currents. The first suggestion of the existence of turbulent motions in stellar atmospheres came from such reasoning. ROSSELAND (1929) commented that stellar rotation exists; computed a Reynolds' number based upon the stellar radius as a characteristic length, and the observed rotational velocity, finding the Reynolds' number to be very large; then suggested that on the basis of laboratory experience such a Reynolds' number requires turbulent motion in the stellar atmosphere. Such a first-approximation procedure must be refined for a detailed quantitative treatment; we are not aware of such a more refined discussion; such must be carried out before much can be said about the properties of any turbulent motion to be expected to accompany stellar rotation.

A similar background for the introduction of an empirical «astronomical turbulence» must be mentioned. The concept arose in considerations of the state of the solar chromosphere, although similar arguments have been applied to the atmospheres of certain eclipsing stars. In the solar chromosphere, the change of intensity with height of many emission lines is observed to lie an order of magnitude lower than the isothermal density gradient corresponding to a temperature associated with the continuous distribution of energy in the optical spectrum. Under the classical stellar model, the temperature of the atmosphere decreases monotonically outward. Thus, these emission gradients were identified with atmospheric density gradients, and an «astronomical turbulence» postulated, which maintained this low density gradient without, however, coupling energetically to the thermodynamic state of the atmosphere (McCREA, 1933). Since the velocity of such «turbulence» is highly super-thermic, the suggestion is hardly tenable from a standpoint of physical consistency (CHANDRASEKHAR, 1934; THOMAS, 1948). The necessity for such a construction has now been removed, by applying an analysis of the observational material from the standpoint of non-equilibrium-thermodynamics. The emission gradients have been shown to differ from the density gradient, and the actual density gradient has been inferred and shown to satisfy hydrostatic equilibrium without introducing any «turbulence», astronomical or otherwise (cf., the summary of work on this point by THOMAS and ATHAY, 1961).



Similar analyses must be applied to other cases in which this concept of «astronomical turbulence» has been invoked to explain apparently anomalous density gradients, before such suggestions can be taken as serious evidence for the existence of this kind of «astronomical turbulence» in a stellar atmosphere.

4) *Wholly theoretical inference, for which there is apparently some indirect observational confirmation.* — The best example is the inferred existence of vertical convection in the lower solar photosphere. One computes the existence of a zone of radiative instability arising from the ionization of hydrogen and helium (UNSÖLD, 1930), (C. PECKER, 1953), (also cf. the recent summary by J-C. PECKER, 1959) thus infers the onset of convection. The *existence* of solar granulation *appears* to confirm the theoretical result. The problem is to obtain sufficiently detailed astronomical observations, and a sufficiently complete physical theory of such convection in the stellar-atmospheric environment, that the two may be compared in detail. The comparison involves not only velocity fields, but spectral, and angular (over the solar disk), distributions of radiation. We defer consideration of such phenomena to the detailed treatment in the Symposium proceedings Part IV-A.

Of these four kinds of inferential procedures, 2) has provided by far the greatest body of astrophysical results. Thus, we concentrate our discussion upon it.

## 2. — A quick look at the conventional astronomical approach to the analysis of spectral lines.

In discussing the analysis of a stellar spectral line, we will at times emphasize questions relating to a difference of only a few percent in the intensity at some point on the line-profile, insisting on the critical nature of this apparently-small difference for a determination of velocity fields. The velocity we have to determine is obtained by comparison of «*a*» theoretical line-profile and «*the*» observed one. Because the differences between almost any theoretical line-profile and the observed one are usually not gross, it is essentially the situation that a *first-order* approximation on the determination of the velocity requires a *second-order* approximation on the theory of the line-profile. Such a second-order approximation requires a knowledge of the chemical composition of the atmosphere, and of the distribution through the atmosphere of temperature and occupation numbers of energy levels (as given by thermodynamic equilibrium distribution functions or not). Very often, astrophysical

work has been limited to the first-order approximation (constant temperature and root-mean-square value of velocity, thermodynamic equilibrium relations), which has given valuable information for the investigation of differences of chemical composition between different kinds of stars, and of stellar evolution.

Put this another way. We can try to distinguish the interest of three kinds of people, in these questions of velocity fields, at this symposium. One group are those astrophysicists whose primary concern lies in questions of stellar composition and evolution, and who wish to eliminate as far as possible a consideration of the details of physical processes entering line-formation except insofar as they make a large difference in results on composition. A second group, on the other extreme, are aerodynamicists who would like the fullest possible information on aerodynamic phenomena occurring in stellar atmospheres, as a possible extension of experience from terrestrial laboratories, and with not much concern for details of stellar evolution except insofar as they bear on aerodynamic questions. Finally, there is an intermediate group, astrophysicists-physicists, whose primary interest lies simply in the physical problems associated with the interaction between radiation and velocity fields, and thus interested in the fullest possible attention to details of line-formation. For the first category of interests, it often appears sufficient to deal with the total energy in the spectral line, ignoring details of the line-profile, particularly since they wish to deal with large numbers of stars, some quite faint, for which detailed spectra are unavailable. The other two categories require a detailed consideration of line-structure, particularly in the very central regions of the line, where velocity fields have their maximum influence upon the absorption coefficient. Clearly, our discussion here must aim primarily at the two latter categories, and to a large extent deal with problems of the kind of analysis which should be applied, even though only few of the necessary data may be presently available. However, we introduce the discussion by a glossary of standard astrophysical terminology, and an introductory first-order physical exposition, which essentially reflects the viewpoint of the first category above.

## 2.1. Glossary of terminology.

1) The spectral line represents a transition between energy levels, whose occupation numbers we denote by  $n_2$  (upper level) and  $n_1$  (lower). The spontaneous transition probability between levels is  $A_{21}$ .

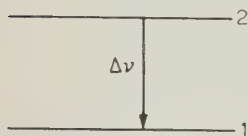


Fig. 1. -- Schematic transition.

We use subscript «c» to denote continuum, «s» to denote selective process in the line, and «v» to represent the combined line and continuum, which is the observed quantity.  $n_k$  denotes a concentration;  $N_k$  denotes an integral over some path length, thus number/cm<sup>2</sup>.



- 2)  $I_\nu$  = specific intensity ( $\text{erg cm}^{-2} \text{s}^{-1} \nu^{-1}$  solid angle $^{-1}$ ) at frequency  $\nu$ .  
 $I_c$  = specific intensity in the continuum immediately adjacent to the line.  
 $R_\nu = I_c - I_\nu$ .  
 $j_\nu$  = emission coefficient per atom in upper level ( $\text{erg s}^{-1} \nu^{-1}$ ).  
 $\alpha_\nu$  = absorption coefficient per atom in lower level ( $\text{cm}^{-2}$ ).  
 $\varphi_\nu$  = profile of absorption coefficient such that  $\int \varphi_\nu d\nu = 1$ .  
 $\alpha'_\nu = \alpha_\nu$  including stimulated emission (similar definitions with the index  $\lambda$ ).

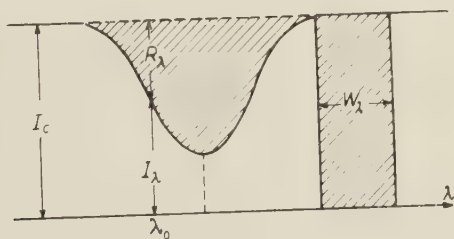


Fig. 2. - Notation for the spectral line (indices  $\lambda$  can be replaced by indices  $\nu$ ).

- 3) For a Doppler profile of absorption coefficient (only velocity broadening)

$$\alpha_\nu = \frac{\pi e^2}{m_e c} f_{12} F(V) dV, \quad f_{12} = \text{oscillator strength}, \quad \int F dV = 1,$$

$$\nu = \nu_0(1 + V/c), \quad \text{subscript 0 refers to line center},$$

$F(V)$  is the local velocity distribution. If  $F(V)$  is Gaussian — *i.e.* random velocity distribution:

$$\alpha_\nu = \frac{\pi e^2}{m_e c} f_{12} \frac{1}{\sqrt{\pi} \Delta \nu_D} \exp[-(\Delta \nu / \Delta \nu_D)^2],$$

$$\Delta \nu_D = (\nu_0/c)(2\overline{V^2})^{1/2}; \text{ thermal motion—}\overline{V^2} = kT/m_a.$$

- 4) The time-independent equation of radiative transfer in an atmosphere where curvature effects may be neglected is

$$\mu \frac{dI_\nu}{d\tau_\nu} = I_\nu - S_\nu,$$

where  $\tau_\nu$  is called the optical depth, and defined by

$$\tau_\nu = \tau_s + \tau_c, \quad d\tau_s = n_2 \alpha'_\nu dx;$$

$x$  is distance measured along stellar radius;  $\mu = \cos \theta$  where  $\theta$  is the angle between the outward radius and the direction of propagation of the beam; thus as observations on the stellar disk move from center to edge, the value of  $\mu$  for the emergent beam of radiation varies from 1 to 0.

$S$  is called the source-function and defined by

$$S_\nu = \frac{S_s + r_\nu S_c}{1 + r_\nu} = \frac{\eta_\nu S_s + S_c}{\eta_\nu + 1},$$

$$S_s = N_2 j_\nu / N_1 \alpha'_\nu; \quad r_\nu = \eta_\nu^{-1} = d\tau_c / d\tau_s.$$

5) Equivalent width — measures « total absorption » in a line:

$$W_\lambda = \int \frac{(I_c - I_\lambda) d\lambda}{I_c} = \int \frac{R_\lambda d\lambda}{I_c},$$

$$R_\lambda = I_c - I_\lambda,$$

subscripts  $\lambda$  and  $\nu$  on  $I$  and  $R$  denote quantities per unit wave-length or unit frequency, respectively.

6)  $B_\nu(T_e)$  = Planck function for electron temperature,  $T_e$ .

7)  $\varepsilon$  = ratio of rates of collisional to radiative de-excitation evaluated at the local values of  $n_e$  and  $T_e$  under conditions of local thermodynamic equilibrium.

$\eta B^*$  = ratio of rates of radiative ionization from the lower level to spontaneous downward transition in the line.

$\eta$  = ratio of rates of radiative ionization from upper level to spontaneous downward transition in the line.

8)  $B(\Delta\lambda)$  = broadening function used by Huang and Struve due to macroscopic mass motion.

$R'(\lambda)$  = physical Doppler broadening function used by Huang and Struve.

$R(\lambda)$  = geometrical Doppler broadening function used by Huang and Struve.

9) Note that the following pairs of quantities should not be confused; they have nothing to do with each other:

$$\eta \text{ and } \eta_\nu; \quad B_\nu(T_e), B^*, \text{ and } B(\Delta\lambda); \quad R_\lambda \text{ and } R(\lambda).$$

We have simply followed notation used in the astronomical literature for ease of supplementing this article with references. We have changed one



em of notation —  $S_s$  replacing  $S_L$  used in the literature for the source-function in the line.

**2'2. Total energy in the line — rough approach.** — In a rough, semi-quantitative way, we distinguish the manner in which three major effects enter to determine the total energy carried by the line. In Fig. 3, we plot

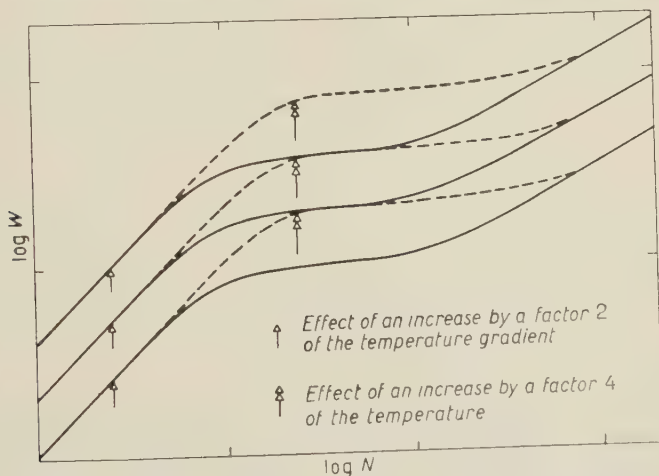


Fig. 3. — Influence of  $T$ ,  $dT/dh$  on total absorption using LTE relations.

the behavior of the total integrated absorption (ordinate) as a function of the abundance of atoms in the ground level (abscissa), with differing curves corresponding to differing values of the temperature gradient in the atmosphere, and to differing kinetic temperature (or other very small-scale random motion). We see from the figure that an increase in  $dT/dh$  by a factor 2 increases the equivalent width,  $W$ , by the same factor; and that this same effect requires an increase by a factor 4 in the kinetic temperature. We make these computations on the basis of the assumed applicability of thermodynamic equilibrium relations.

Generally speaking, measurements of the distribution of energy in the continuous spectrum have given relatively fair knowledge of the temperature of the atmosphere, using classical equilibrium thermodynamics. These temperatures have been used to compute the theoretical intensity of the line, and the differences between this theory and observations have been laid to velocity fields without questioning the underlying hypotheses. (We will be primarily concerned in Section 3 with the validity of these hypotheses.)

**2'3. Profile of a line — rough approach.** — Consider the first integral of the equation of transfer, in a semi-infinite atmosphere, under the assumption

that  $S_\nu$  does not increase inward as fast as  $\exp[\tau_\nu]$ . This integral gives the emergent specific intensity at some position,  $\mu$ , on the stellar disk as

$$(1) \quad I_\nu(\tau_\nu = 0, \mu) = \int_0^\infty S_\nu \exp[-\tau_\nu/\mu] d\tau_\nu/\mu.$$

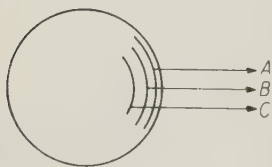
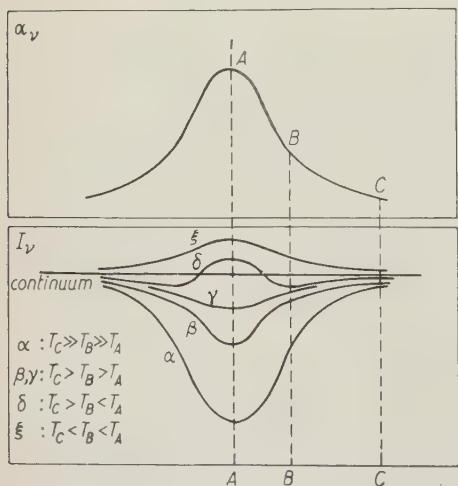
(The restriction on the inward rate of increase of  $S_\nu$  is not a serious one, but we do not discuss its justification here.) Consider now the situation where local thermodynamic equilibrium, LTE, is satisfied so that

$$(2) \quad S_\nu = B_\nu(T_e),$$

Then, because of the exponential term in eq. (1), we have, roughly

$$(3) \quad I_\nu(0, \mu) \sim S_\nu(\tau_\nu \sim \mu) \sim B_\nu(T_e[\tau_\nu \sim \mu]).$$

Using this rough relation, we see the structure underlying the analysis of the total absorption in the line, summarized in Section 2; and also see how the more extensive analysis permitted by the line-profile gives us more detailed information about the atmosphere.



First, the relation (3) demonstrates that the form of the line-profile simply reflects  $dT_e/d\tau$ —for the line-profile simply corresponds to looking to different depths in the atmosphere in the line and the continuum, deeper in the continuum than in the line. Thus,  $T_e$  decreasing outward gives an absorption line; increasing outward, an emission line (cf. Fig. 4). A line with a greater ratio  $\tau_s/\tau_c$  gives a more pronounced absorption, a *deeper* line, greater  $R_\nu$  (for  $dT_e/d\tau_\nu > 0$ ). Thus, we see the effect of both model (change in value

Fig. 4. — The influence of the temperature gradient on the profile of a line (schematic).



of  $dT_e/d\tau$ ) and of abundance (value of  $\tau_s/\tau_i$ ), as already illustrated in Section 2 for total absorption in the line.

Second, the relation (3) gives a method for obtaining an empirical model,  $T_e(\tau)$ , for the atmosphere. It gives the mapping of  $T_e(\tau_r)$  from  $I_\nu(\mu)$  for every point, or value of  $\nu$ , on the line. Each point covers a range in  $\tau_r$  equal to the range in  $\mu$ , or about a factor 10, since astronomical observations generally cover the range  $1 > \mu > 0.1$ . Observations of the continuum let us map the deeper layers; of the lines, the higher layers. Thus, we obtain a series of line-segments, each giving  $T_e(\tau)$  over a limited portion of the atmosphere. If we know, a priori,  $d\tau_r/d\tau_s$ , or  $d\tau_{r1}/d\tau_{r2}$ , we can directly relate the segments determined from observations at the several  $\nu_1$ . Such an *a priori* knowledge requires two kinds of information: knowledge of the  $\nu$ -dependence of the absorption coefficient; knowledge of the relative concentrations in the lower levels of the transitions considered. The relative concentrations depend upon both abundance of atom considered, and distribution over excited energy states; in the LTE case, the latter is specified by the LTE distribution functions, leaving only abundance as a free parameter. Thus, for the sun, the only star whose disk we can resolve, we can use line-profiles to get abundance by forcing agreement of the  $T_e(\tau_r)$  segments determined from different ions.

Third, because we can use both  $I_\nu(\mu)$ , and  $I(\nu)$  for a fixed  $\mu$ , to infer  $T_e$ , at different depths, we have an empirical method of investigating the  $\nu$ -dependence of  $\tau_\nu$ , and thus the  $\nu$ -dependence of  $\alpha_\nu$ . In the central regions of the line,  $\alpha_\nu$  has a frequency-dependence fixed by the velocity field; thus, an empirical result for the  $\nu$ -dependence of  $\alpha_\nu$  gives an empirical measure of the velocity field.

In Section 3, we comment in more detail on each of these points, together with their validity, which rests upon equation (2). Here, however, we have tried only to give a rough picture of the usual general approach to the astrophysical analysis of a spectral line.

### 3. - Critical look at the general astronomical methodology.

In the structure of this discussion, we want to do two things. On the one hand, we want to make clear the difference between the factors influencing the shape of the line-profile in the usual astrophysical situation, where one

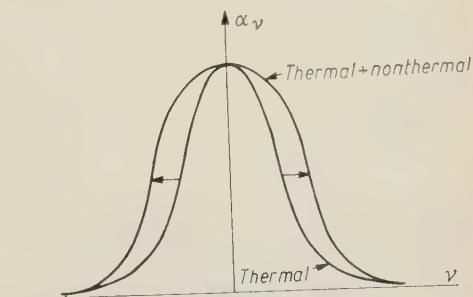


Fig. 5. - Effect of a microscopic velocity field on the absorption coefficient.

generally studies an optically-thick gas, and those factors in the usual laboratory situation, where one generally studies an optically-thin gas. On the other hand, we want to make clear the physics underlying the analytical procedure for the interpretation of line-profiles *as it has thus far evolved* in astrophysics. In such a way, we hope to make clear the basis upon which astrophysical knowledge of velocity fields rests, and also the problems faced in applying the analytical techniques developed in astrophysics to the situations encountered in high-energy aerodynamics and laboratory plasma physics.

We proceed to this analysis in four stages: *A)* a comparison of the laboratory and astrophysical situations with respect to line-profile formation; *B)* analysis of a line-profile formed in an atmosphere where the velocity fields are wholly thermal; *C)* analysis of a spectral line formed in an atmosphere where the velocity fields existing are those in which the *variations* of the velocity, over any scale larger than some dimension much smaller than one photon free path, are completely uncorrelated; *D)* analysis of a spectral line formed in an atmosphere where the velocity fields are of any other type. Because we concentrate on the methodology, deferring results to the several presentations at Varenna, the reader may wish to consult more specific references. We give these in the text on detailed points. As more general summary-type references, we suggest the articles by K. O. WRIGHT (1955), C. DE JAGER (1959), S.-S. HUANG and O. STRUVE (1960). The first summarizes detailed numerical results on random velocities inferred from total line-absorption; the second, summarizes solar material; the last gives a methodological critique and bibliography on empirical results on «stellar turbulence».

3'1. *Astrophysical vs. laboratory situations.* — Consider a small sample of radiating gas in the laboratory, whose optical thickness is negligible, and within which those quantities fixing the frequency-profile of the emission coefficient are independent of position. Then it will produce an emission line; whose specific intensity will be

$$(4) \quad I_{\nu} = N_2 j_{\nu} A_{21} / 4\pi .$$

That is, the frequency-profile of the line will be given by the frequency-profile of the emission coefficient,  $j_{\nu}$ . If we know from theoretical considerations the dependence of  $j_{\nu}$  upon velocity field, then the profile gives us directly the velocity. Indeed, if we consider a thin atmosphere, the profile of  $j_{\nu}$  is just the Doppler profile, so the first-order dependence of the line-profile is upon the velocity field. Thus, the analysis of an observed profile for velocity of emitting atoms is reasonably straightforward.

Precisely the same situation holds, if we consider an absorption line formed by passing a light beam through a thin layer of gas. In this case, the

absorption coefficient  $\alpha_\nu$  replaces the emission coefficient  $j_\nu$ , and its profile is fixed by the velocity field. The observed spectral line-profile is then the profile of  $\alpha_\nu$ .

Consider, by contrast, the astronomical situation, where except in unusual circumstances the expression for the line-profile comes from the first-integral of the equation of radiative transfer, in an atmosphere where curvature effects can be neglected and where the optical opacity along the line of sight is large; viz.,

$$(5) \quad I_\nu(0, \mu) = \int_0^{\tau_{\nu(\max)}} S_\nu \exp[-\tau_\nu/\mu] d\tau_\nu/\mu,$$

assuming either no incident radiation at  $\tau_{\nu(\max)}$  or, in the most usual case of direct observations of the stellar disk, where  $\tau_{\nu(\max)} = \infty$ , that  $S_\nu$  does not increase inward as fast as  $\exp[\tau_\nu]$ , and eq. (5) becomes eq. (1). There are exceptional cases in astrophysics where the situation reduces to the laboratory case of the thin atmosphere, in which case the integral of eq. (5) becomes

$$(6) \quad I_\nu = \int_0^{\tau_\nu} S_\nu \exp[-\tau_\nu/\mu] d\tau_\nu/\mu \xrightarrow{\tau \rightarrow \text{small}} \int_0^{\tau_\nu} S_\nu d\tau_\nu = A_{21} \int_0^{y(\max)} j_\nu n_2 dy/4\pi,$$

which is eq. (4) in an atmosphere where  $j_\nu$  and  $n_2$  vary with position. Examples are gaseous nebulae, atmospheres viewed tangentially as at a solar eclipse, etc. We consider here, however, the most usual case where  $\tau \ll 1$ .

Then, we see the essential character of the problem that plagues the working astronomer—the line-profile represents an integration over the optical depth variation of the source-function,  $S_\nu$ , and  $S_\nu$  is in general not constant even if the quantities fixing the frequency profiles of absorption and emission coefficients are constant throughout the atmosphere. That is, the first-order dependence of the observed line-profile is not upon  $\alpha(\nu)$ —or upon  $j(\nu)$ —but upon  $S_\nu(\tau, \nu)$ . Thus, there are two kinds of dependence of the line-profile upon thermodynamic quantities in the atmosphere: (i) the dependence of profile upon *gradient* of  $S_\nu$  in the atmosphere through the two relations  $S(\tau)$  and  $\tau(\nu)$ —the last being equivalent to  $\alpha(\nu)$  and (ii) the dependence of profile upon any  $\nu$ -dependence of  $S$  (at a particular point in the atmosphere). If we could invert the integral in eq. (5) to obtain directly  $S(\tau)$  and  $\alpha(\nu)$ , knowledge of  $\alpha(\nu)$  would give us directly the total velocity field, precisely as in the laboratory case corresponding to eq. (4) and the simplified astrophysical case corresponding to eq. (6). Then, in both laboratory and astrophysical cases, we must separate thermal from non-thermal components. The possibility of inferring  $T_e$  in the laboratory case lies in a discussion of  $N_2$ ; in the astrophysical



case, of  $S_\nu$ . Thus, in the astrophysical case, the problem is to analyse the line-profile in such a way as to invert eq. (3) to obtain both  $S_\nu(\tau_\nu)$  and  $\alpha(\nu)$ .  $\alpha(\nu)$  gives total velocity field, and we must ask the relation between the value of  $S_\nu$  and the value of  $T_e$ .

A precise parallel to the laboratory case would arise if we were able to by-pass the determination of  $S(\tau)$  and determine directly  $\alpha(\nu)$ . We first summarize an approach to such a procedure, which has been used in astrophysics but is actually only valid under certain highly restrictive assumptions, then consider the more general case where  $\alpha(\nu)$  cannot be determined without prior or simultaneous determination of  $S(\tau)$ .

**3'1.1. Direct determination of  $\alpha(\nu)$ .** The following method has been used by several authors (cf. DE JAGER, 1952; ATHAY and THOMAS,

1957, 1958, who critically examined its use; GOLDBERG, 1958; UNNO, 1959). One observes several lines originating on a common lower level, then proceeds to analyse the observations under the two assumptions that  $S_\nu$  is  $\nu$ -independent over a given line, and is the same for all the lines considered. Observations of the line-profiles are compared at the same point on the solar disk. Find two points of equal intensity, one on each line. We have  $I_{\nu_i} = I'_{\nu_j}$  (distinguishing the lines simply by primed

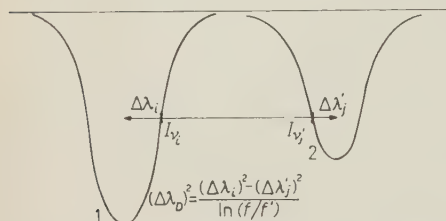


Fig. 6. Intercomparison of two spectral lines to obtain  $\Delta\lambda_D$ , assuming a common  $\nu$ -independent  $S_\nu$ : Gaussian distribution of velocities.

and unprimed notation cf. Fig. 6). Since  $S_\nu$  is the same for both lines, consider its functional form expressed in terms of  $\tau_{\nu_i}$ , say. Further define the ratios, at a given geometric point in the atmosphere:

$$(7) \quad d_{\nu_i} \tau / d\tau'_{\nu_j} = \alpha_{\nu_i} / \alpha'_{\nu_j} = \delta; \quad \tau_{\nu_i} / \tau'_{\nu_j} = \Delta,$$

$\Delta$  and  $\delta$  are generally functions of position, of course. Then eq. (1) gives

$$(8) \quad 0 = I_{\nu_i} - I'_{\nu_j} = \int_0^\infty \{S_\nu(\tau_{\nu_i}) \exp[-\tau_{\nu_i}] d\tau_{\nu_i} - S_\nu(\tau'_{\nu_j} \Delta) \exp[-\tau'_{\nu_j}] d\tau'_{\nu_j}\} = \\ = \int_0^\infty S_\nu(\tau_{\nu_i}) \exp[-\tau_{\nu_i}] \{1 - \delta^{-1} \exp[\tau_{\nu_i}(1 - \Delta^{-1})]\} d\tau_{\nu_i}.$$

One solution corresponds to  $\Delta = \delta = 1$ , thus fixes empirically the  $\nu$ -dependence of  $\alpha$ . This requires constancy through the atmosphere of  $\alpha_\nu$ —which

requires constancy of  $h\nu_D$  or total velocity. This solution is not the only one, but it is the only one that has been used thus far in astrophysical practice, as some kind of first approximation. Other solutions correspond to variable velocity fields, and can be obtained only iteratively.

This procedure depends upon the assumption that  $S_\nu$  is both  $\nu$ -dependent, and the same for several lines. The assumption of LTE, eq. (2), satisfies this criterion, and is sometimes adopted (cf. NEVEN and DE JAGER, 1954). Other authors have not insisted upon the applicability of LTE, but have taken it as obvious that lines originating on a common lower level, and having not too great an energetic separation of upper levels, will satisfy the condition of a common  $S_\nu$  (cf. GOLDBERG, MOHLER, MUELLER, 1959; UNNO, 1959). We remark only that the validity of these assumptions remains to be investigated in each case. We turn in Sect. 3'2 to the question of what form  $S_\nu$  should have in reality.

**3'1.2. Joint determination of  $S(\tau)$  and  $\alpha(\nu)$ .** In the general case, we ask first whether we may not avoid the uncertainty of assumption on relation between  $S_\nu$  for several lines by working at several points on one line, looking at the variation of line-profile across the disk to obtain  $S_\nu(\tau_\nu)$ . The major uncertainty accompanying such a procedure arises in the possibility of departure of the atmosphere from spherical symmetry. We set this question aside, for the moment, and proceed under the assumption of spherical symmetry.

The difficulty in inverting eq. (5) to obtain  $S_\nu(\tau_\nu)$  from an observed  $I_\nu(\mu)$  is often so great as to divert the astrophysical analysis from the recognition that any physical interpretation of the results can *only* be made on the basis of a set of *local* values—of  $S_\nu$ , of absorption coefficient, of occupation numbers—values referring to a particular point in the atmosphere. In the case of the solar atmosphere, and modern equipment, one has reasonably-good success in actually observing  $I_\nu(\mu)$  with good resolution in both  $\nu$  and  $\mu$ , then using  $I_\nu(\mu)$  to obtain  $S_\nu(\tau_\nu)$ . In the stellar case, however, one always treats observations referring to the stellar disk as a whole—integration over  $\mu$ —and often results must be based only on total absorption in the line—integration over  $\nu$ —because of the poor spectral resolution associated with a faint object. In such cases, passage from the observed integrated quantities to *local* quantities is quite difficult (physical interpretation of numerical values of *local* quantities can only come later).

To interpret local values of  $S_\nu$ , one asks two questions: (i) how much does the numerical value of  $S_\nu$  depend upon the local value of  $T_\nu$ , and how much directly upon the velocity field present; (ii) how can we use the empirical set of  $S_\nu(\tau_\nu)$  to infer the  $\nu$ -dependence of  $\alpha_\nu$ , for this last fixes the velocity field just as in the laboratory case. We consider first the second question, then consider the first question in Section 3'2.

We ask now what procedure we may follow to interpret the set of empirical values,  $S_v(\tau_v)$ , to find thermal and non-thermal components of any atmospheric velocity fields. The procedure to be adopted depends very much upon the way  $S_v$  depends upon the thermodynamic parameters characterizing the state of the atmosphere. Since in this subsection we are trying to compare astrophysical and laboratory situations, we introduce a second idealization, the very specialized form of  $S_v$  holding under conditions of LTE; viz.,  $S_v$  given by eq. (2). From this specialized example, we try to make clear what characteristics of  $S_v$  enter which part of the analysis, then in Sections 3'2-3'4 we may ask into the form expected for  $S_v$  from physical considerations, thus the relation of  $S_v$  to  $T_e$  and velocity fields. It should be noted that the results of this specialized form are not only of interest for illustrative purposes. We have already remarked that astronomical analyses have often been so highly preoccupied with the difficult problem of inverting the integration problem to obtain  $S_v(\tau_v)$ , that they make oversimplified assumption on the interpretation of  $S_v$ . This assumption usually takes the LTE form of eq. (2). Consider, then, how we obtain thermal and non-thermal velocity fields under this assumption.

Again to eliminate consideration of details of inverting eq. (5) at this point, and to permit us to focus attention on the essential characteristics of the joint analysis for  $S(\tau)$  and  $\alpha(v)$ , we introduce another specialized assumption, that permits us to pass directly from a numerical value of  $I_v(\mu)$  to a numerical value of  $S(\tau)$ , and links our systematic discussion to the rough analysis of Section 2'3. We assume

$$(9) \quad S_v(\tau_v) = a_v + b_v \tau_v.$$

Then for  $\tau_{v(\max)} \gg 1$ , eq. (3) leads immediately to

$$(10) \quad I_v(\mu) = a_v + b_v \mu = S_v(\tau_v = \mu).$$

In such a case, as noted in Section 2'3, an observed set of values  $I_v(\mu)$  suffices to map out  $S_v(\tau_v)$  over the range  $1 > \tau_v > \mu$  (min). There is no a priori reason to expect such linearity for  $S_v(\tau_v)$ , but over a limited range in  $\tau_v$  it is often nearly true. As mentioned, we use the assumption here simply for illustrative purposes, then comment on the problems its use introduces.

Given  $S_v = B_{v_0}(T_e)$ , eq. (10) permits a mapping of the distribution of  $T_e$  in the atmosphere as a series of segments,  $T_e(\tau_v)$ , one segment for each point on the line-profile, over the permitted range in  $\tau_v$ . If one knew the relation between  $\tau_{v_i}$  and  $\tau_{v_j}$  (i.e. the  $v$ -dependence of  $\alpha_v$ ), where  $v_i$  and  $v_j$  are two points on the line-profile, the several segments could be combined to give  $T_e(\tau_{v_0})$ , say, over a large range in  $\tau_{v_0}$ . Thus, one would map out the *thermal* velocity



## Quote sociali e prezzi di abbonamento alle pubblicazioni

### ● Quote sociali per l'anno 1962.

Per l'Italia:

Soci individuali . . . .	L. 9 000
» collettivi . . . . »	25 000
» sostenitori . . . . »	60 000

Per l'Estero:

Soci individuali . . . .	\$ 18 (L. 11 250)
e collettivi . . . .	
Soci sostenitori . . . .	\$ 96 ( » 60 000)

Ai Soci vengono inviati gratuitamente *Il Nuovo Cimento*, il *Supplemento al Nuovo Cimento* e il *Bollettino della Società Italiana di Fisica*.

### ● Abbonamento al *Nuovo Cimento* e al *Supplemento al Nuovo Cimento* per l'anno 1962, per enti e persone *non* Soci della Società Italiana di Fisica.

Per l'Italia:

abbonam. ordinario . .	L. 15 000
» sostenitore . . »	60 000

Per l'Estero:

abbonam. ordinario . .	\$ 28 (L. 17 500)
sostenitore . .	\$ 96 ( » 60 000)

Agli Abbonati viene inviato gratuitamente il *Bollettino della Società Italiana di Fisica*.

#### EDIZIONE IN CARTA NORMALE E IN CARTA LEGGERA.

Il *Nuovo Cimento* e il *Supplemento* si pubblicano in due edizioni: una in carta normale e una, in ristretto numero di copie, in carta leggera (India bianca). Il prezzo è il medesimo per le due edizioni se spedite per posta normale. Salvo richiesta contraria, si spedisce l'edizione in carta normale.

Coloro che desiderino ricevere l'edizione in carta leggera per posta aerea dovranno assumersi le maggiori spese del trasporto aereo. L'ammontare di dette spese può ricavarsi considerando che l'edizione per il 1962 in carta leggera peserà in totale (*Nuovo Cimento* e *Supplemento* circa  $(4 \div 4,5)$  kg.

Gli autori sono pregati d'inviare, a fare data dal 1° Gennaio 1962, i propri lavori per la pubblicazione al seguente indirizzo: Direttore del NUOVO CIMENTO, Bologna, Via Irnerio, 46, presso l'Istituto di Fisica dell'Università.

## Membership fees and subscription fees to publications

### ● Membership fees for the year 1962.

For Italy:

Individual Members . Lit. 9 000

Collective » » 25 000

Sponsoring » » 60 000

For Foreign Countries:

Individual and

Collective Members. \$ 18 (Lit. 11 250)

Sponsoring Members. \$ 96 (Lit. 60 000)

To Members *Il Nuovo Cimento*, the *Supplemento al Nuovo Cimento* and the *Bollettino della Società Italiana di Fisica* are sent free of charge.

### ● Subscription fees to the *Nuovo Cimento* and to the *Supplemento al Nuovo Cimento* for the year 1962 for persons and Institutions *not being* Members of the Italian Physical Society.

For Italy:

ordinary subscription. Lit. 15 000

sponsoring » » 60 000

For Foreign Countries:

ordinary subscription \$ 28 (Lit. 17 500)

sponsoring » \$ 96 (Lit. 60 000)

To subscribers the *Bollettino della Società Italiana di Fisica* is sent free of charge.

### NORMAL PAPER EDITION AND WHITE INDIA PAPER EDITION.

The *Nuovo Cimento* and the *Supplemento* are published in two editions: one on normal paper and the other, in only a limited number of copies, on light paper (white India). The price is the same for both editions when they are shipped by surface mail. Unless instructions to the contrary are given, it is the normal paper edition which will be shipped.

Subscribers who wish to receive the light paper edition by air mail will be charged with the necessary additional postal fees. The amount of these expenses may be gathered from the fact that the white India paper edition (*Nuovo Cimento* and *Supplemento*) will assume for 1962 the total weight of (4 + 4,5) kg.

Authors are kindly requested to send, to begin with Jan. 1st. 1962, their papers submitted for publication to the following address: Direttore del NUOVO CIMENTO, c/o Istituto di Fisica dell'Università, Via Irnerio, 46, Bologna.

field over the atmosphere, provided the assumptions of eq. (2) and (9) are satisfied. ATHAY and THOMAS (1955) have discussed some of the difficulties of such an analysis in the case of the Balmer lines of hydrogen; CURY, LEFEVRE and PECKER are currently carrying out a somewhat more extensive investigation of the problem for Ti. (*Ed. note:* cf. remarks by PECKER, Part I, Discussion).

A knowledge of the relation between  $\tau_{v_i}$  and  $\tau_{v_j}$  must come either from a theoretical calculation, or from some empirical procedure. Such a theoretical calculation depends upon an *a priori* knowledge of the general atmospheric velocity field, in order to compute the  $v$ -variation of  $\alpha_v$ . Since we do not have this *a priori* knowledge, one requires an empirical relation between  $\tau_{v_i}$  and  $\tau_{v_j}$ , which is, of course, equivalent to an empirical investigation of the  $v$ -variation of  $\alpha_v$ , this in turn being the basis for establishing the characteristics of any existing velocity field. Thus, by establishing the  $v$ -variation of  $\alpha_v$ , we are able to join the segments to produce an overall  $T_e(\tau)$ , thus extending our knowledge of the thermal velocity field over that part of the atmosphere contributing to the observed line, and also measure the total velocity-field in the same atmospheric region.

Again, the LTE assumption permits an inference of the required  $v$ -variation of  $\alpha_v$  directly from the empirical data in the following way. Since  $S_v$  is, under this assumption, independent of  $v$  over the line, the observed  $v$ -variation of  $I_v$  simply reflects a combination of the depth variation of  $S_v$  and the  $v$ -variation of  $\alpha_v$ . If we find points  $\mu_1$  and  $\mu_2$  on the solar disk such that  $I_{v_i}(\mu_1) = I_{v_i}(\mu_2)$ , the eq. (6) implies that  $\tau_{v_i} = \mu_1$  and  $\tau_{v_j} = \mu_2$  refer to the same geometrical point in the atmosphere. (Unless  $T_e$  does not vary monotonically with height, but this can be determined empirically, and does not introduce complication.) Thus, we have

$$(11) \quad 0 = \tau_{v_i}/\mu_1 - \tau_{v_j}/\mu_2 = \int_{\lambda}^{\infty} n_1(\alpha_{v_i}/\mu_1 - \alpha_{v_j}/\mu_2) dx \rightarrow \alpha_{v_i}/\alpha_{v_j} = \mu_1/\mu_2.$$

Provided we have a sufficiently-detailed and accurate set of measures of  $I_v(\mu)$ , over the line-profile, this procedure suffices to give an empirical evaluation of the  $v$ -dependence of  $\alpha_v$ . Since we know how  $\alpha_v$  varies in the presence of a velocity field, we invert the empirical relation to infer the velocity field. Our knowledge of  $T_e(\tau_v)$  permits us to separate out the thermal and non-thermal components.

Turn now to consider the effect of the simplifying assumptions represented by, respectively, eq. (2) and (9), upon the results just obtained. Eq. (2) is LTE; eq. (9) is the linearity of  $S_v(\tau_v)$ . We consider first the effect of eq. (9). Eq. (10) implies the highly restricted result that  $\alpha_{v_i}/\alpha_{v_j}$  is constant throughout the atmosphere; viz, the effect of the velocity field on  $\alpha_v$  is constant throughout



the atmosphere. This limitation is not particularly serious for our present purpose of comparison between thin gas in a laboratory, and stellar atmosphere: for it corresponds to the condition of eq. (4). We should, however, recognize the implication of the result in a discussion of astrophysical methodology. The result is a consequence of the assumption (9) applied to a  $\nu$ -independent  $S_\nu$ : it can readily be shown that these two conditions combine to require a constant value of  $\alpha_{\nu_i}/\alpha_{\nu_j}$ . Thus, when discussing a  $\nu$ -independent  $S_\nu$ , we recognize that only the non-linear terms in  $S_\nu(\tau_\nu)$  contain information on the height-gradient of the atmospheric velocity field. We make this point, because the relation of eq. (8) is often extremely convenient to use in practice to make a first-estimate of  $S_\nu(\tau_\nu)$ . More refined analysis often appears to show only small change from this first-approximation result; *e.g.*, at  $\mu=1$ ,  $I_\nu$  for a particular  $\nu$  may correspond to  $S_\nu$  at  $\tau_\nu=0.7$ , rather than  $\tau_\nu=1$  as required by eq. (9). It is, however, these small differences that are important in specifying the detailed behavior of the velocity field. It is often necessary to discuss the concept of *effective depth of formation* of a line, particularly when one does not have available a complete set of  $I_\nu(\mu)$  as data, but some integrated form of these. Under such circumstances, it is important to keep in mind the points we have made here.

Turn now to the second simplifying assumption, LTE given by eq. (2), and ask what it really does, in the way of fixing the analytical procedure. First, the assumption requires the *local* value of  $S_\nu$  to depend *only* upon the *local* value of  $T_e$ . Thus, the empirical value of  $S_\nu$  fixes immediately the thermal velocity field, but it has no direct connection with the non-thermal velocity field. Second, the assumption requires  $S_\nu$  to be frequency-independent over the line. Consequently, the observed line-profile becomes immediately translated into the  $\nu$ -dependence of  $\alpha_\nu$ , through the intermediary of the depth-dependence of  $S_\nu$ . In a sense, the LTE assumption reduces the stellar-atmosphere case to an «equivalent» thin-atmosphere case. It accomplishes this by reducing  $S_\nu$  to a quantity characteristic of the line as a whole, suppressing its  $\nu$ -dependence, with magnitude fixed by the local thermal velocity; and by restricting the influence of the macroscopic velocity field wholly to  $\alpha_\nu$ , whose variation with  $\nu$  translates the depth-dependence of  $S_\nu$  into the observed profile of the line.

When, then, we turn to consider the actual physical expectation on the form of  $S_\nu$  in the stellar atmosphere, these considerations suggest that our primary attention lie on two questions:

(i) To what extent can  $S_\nu$  be considered to be a quantity characteristic of the line as a whole? That is, how strong is its  $\nu$ -dependence? If the dependence is strong, we must expect some radically-different procedure than that outlined above to be necessary to obtain information on atmospheric macroscopic velocity field.

(ii) How strongly-controlled is the local value of  $S_\nu$  by the local value of  $T_e$  in the atmosphere? If control is weak, we cannot expect a good determination of the thermal velocity field.

In Section 3'2, we summarize existing knowledge on  $S_\nu$  from the standpoint of these two questions. Here, we have tried only to emphasize the reasons underlying a serious concern with the form of  $S_\nu$ , and the question of the validity of the LTE assumption, in setting up the methodology for inferring properties of atmospheric velocity fields from observed line-profiles. We turn now to brief comments on the difficulties introduced in the analysis by problems of geometrical and instrumental resolution.

### 3'1.3. Problems of resolution—geometrical and spectral—in the astrophysical case.

*a) Problem of geometrical resolution.* We divide this question into two parts: that encountered in solar physics, because of lack of sufficient resolution to observe details of size less than some  $1''$  of arc, or about 700 km on the solar surface; and the stellar case, where no resolution of the disk is possible at all, and one observes  $F_\nu$  rather than  $I_\nu(\nu)$ .

$\alpha$ ) The solar case. A concern with the fine-structure of the solar case is quite recent, and provides much of the basis for direct inquiry into departure from spherical symmetry. One has concern that there may exist systematic velocity gradients over the solar surface, showing appreciable velocity differences over distances of the order  $1''$  or less. (That is, one is concerned with the existence of horizontal gradient in vertical velocity.) If indeed the line-of-sight velocity differences are large enough to introduce an observable shift in line-position, and the intensity variation over such a shift is comparable with the accuracy of measure of  $I_\nu$ , then a serious systematic effect would be introduced into the interpretation of the  $S_\nu(\tau_\nu)$  relation.

Suppose, for example, one had a situation where there was a system of rising and falling columns of gas, within each of which the only velocity field was thermal. If the instrumental resolution were such that only one column was observed, we could analyse its structure according to the procedures discussed in subsections 3'1.1 and 3'2.2, and in Section 3'2 following. If, however, several such columns were observed together, the observed profile would be the superposition of several profiles, each identical but displaced in wave-length. Clearly the analytical procedures discussed thus far would lead to erroneous results.

To investigate the errors arising from such lack of instrumental resolution, one must compare at the line-center the expected curvature of the theoretical profile for a line in a static atmosphere with the calculated curvature coming from the shift in line-position associated with the horizontal gradient in line-

of-sight velocity. To our knowledge, such an investigation has not yet been made, for any of the lines where preliminary observations with the high

resolution equipment have shown the fine-structure to exist. We regard this investigation as a fundamental one (cf. Fig. 7).

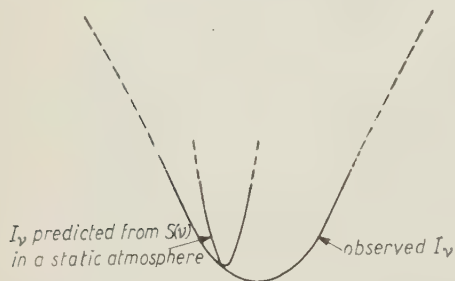


Fig. 7. — Line-profile as superposition of profiles from moving gas columns.

$\beta$ ) The stellar case. Dealing with observations of  $F_\nu$  alone, rather than  $I_\nu(\mu)$ , makes more difficult the process of constructing  $S_\nu(\tau_\nu)$  for each  $\nu$ . Such construction could be accomplished only by the combination of several lines, such as outlined when discussing departures from spherical symmetry. What is usually done in practice, is to assume the va-

lidity of LTE, and a model of the atmosphere so that  $T_e(\tau_c)$  is known, then compare the profile with one constructed assuming thermal velocities alone to be present. Usually, the first approximation to such an approach comes from comparing the total absorption in the line (cf. 3'4 below) with that expected from thermal velocities alone, thus inferring a « turbulent » component of velocity. Then, one compares this « turbulent » velocity inferred from the total absorption with that required to give the observed width of the line-profile. We return to this subject in Sections 3'3 and 3'4, directing particular attention there to the methodology set up by HUANG and STRUVE. Actually few detailed analyses from even this LTE viewpoint have been carried out, mainly because a strong line would be required, and there is considerable uncertainty in the distribution of  $T_e$  in the upper atmospheric layers where such a line would be formed, even under this classical, LTE model. Nothing has been done, to our knowledge, from the standpoint of dropping the LTE assumption and treating the line from the complete non-LTE viewpoint.

b) Problem of spectral resolution. Again, as in Sect. 3'1.2, we divide the question into two parts: the question of very-high resolution spectroscopy in solar work, *vs.* ordinary resolution; and the stellar case of weak lines from faint stars, where only integrated (over  $\nu$ ) profiles are available. These last are treated in terms of the so-called equivalent widths; *viz.*, the width of a line of zero central intensity which would have the same total absorption as the observed line.

The high resolution solar spectroscopy offers the principal hope for detailed investigation of line-structure for really identifying the details of localized velocity fields. The same comment can be made as in 3'1.3, essentially no detailed



work has been based on this kind of material, primarily because little detailed observational material exists as yet.

Analysis in terms of equivalent widths gives information on depth variation of physical quantities only insofar as we can observe different lines having different « effective » depths of formation. Since a primary goal of most astrophysical analyses of stellar spectra lies in determination of abundances, and since the abundance of a given atmospheric constituent determines its depth of formation, one cannot approach the problem of depth-dependence of physical quantities, using equivalent widths alone, from a completely unambiguous viewpoint. This difficulty makes itself felt in a particularly obvious way when we attempt to ask how valid the LTE assumption may be, by comparing an empirically-determined  $S_p$  with  $B_{\nu_0}(T_e)$  in an atmospheric region where some independent measure of  $T_e(\tau_c)$  exists. Use of equivalent widths alone does seem to permit some insight (cf. the recent summary of PECKER, 1959), but the uncertainties are very large compared with those encountered in analyses based on  $I_p(\mu)$  (cf. Chapter 9, THOMAS and ATHAY, 1961). We would simply like to stress here that the question of the proper form for  $S_p$ , the derived abundance, and the derived properties of the velocity field are all ultimately linked. When one has available only such triply integrated quantity as equivalent width of a stellar line, considerable *a priori* theoretical effort must be introduced to give meaningful results on the particular values of the physical quantities applying in the particular case analysed.

With these general comments on the astrophysical methodology providing a comparison with the laboratory situation, we proceed to more specific detail, breaking the summary into the idealized cases of 3'2-3'4. The reason for such a breakdown is both historical and conceptual, this being the structure actually considered in astrophysical analyses; the reasons for this will become clear in the discussion of the methodology.

*3'2 Methods of analysis of the spectrum produced in an atmosphere where the only velocity fields are thermal.* — Our attention is directed at analysis of stellar atmospheric spectra to infer the character of any existing non-thermal velocity fields, as contrasted to the purely thermal velocity field. Consequently, our interest in this case 3'2 centers around its use as a « control », to clarify some of the questions raised in the survey of astrophysical analytical methodology in sect 3'1. Such a control has two aspects. First, there is the wholly theoretical one of answering the *a priori* methodological questions raised in 3'1, which in essence comes down to an inquiry into the form of  $S_p$ . Second, there is the question of the application of these results to situations where earlier analyses may have proceeded on the basis of assumptions not in harmony with the theoretical conclusions on the proper form of  $S_p$ . We should ask whether the inferred velocity fields actually exist, or whether they are simply

the consequence of a bad choice on  $S_\nu$ . Since it is not our object in this paper to survey the actual results obtained in astrophysical analyses, this being left to the detailed summary-introductory papers at Varenna, we can only attempt to indicate the direction of an effect resulting from such a bad choice on  $S_\nu$ , with a few simple examples after we have surveyed the general theoretical expectation for  $S_\nu$ .

In Sect 3'1, we have shown that concern with the form of  $S_\nu$  centers on two points: the  $\nu$ -dependence of  $S_\nu$ ; and the degree to which the local value of  $S_\nu$  is fixed by the local value of  $T_e$ . Our approach in the present Section is: given the values of the local thermodynamic parameters characterizing the atmosphere, what is the theoretical expectation on  $S_\nu$  and how do we analyse the line-profile using this theoretical form for  $S_\nu$  in order to obtain empirical values for  $S_\nu$  and possibly of  $T_e$ . We have in the introduction referred to indirect evidence on the existence of aerodynamic phenomena. Were we to investigate such indirect evidence, our interest would center on the relation between  $T_e$  and the local radiation field, with respect to the existence of cyclic processes associated with a non-radiative energy supply such as might come from local dissipation of energy from a macroscopic velocity field. Here, however, we simply take the local values of  $T_e$  and other thermodynamic parameters as given, not asking how these values were fixed, then ask what values of  $S_\nu$  are consistent with them, in order to formulate the methodology to invert this procedure.

**3'2.1 The form of  $S_\nu$ .** For the discussion of physical expectation, it is essential to break up  $S_\nu$  into contributions from the continuum and from the line, since the contributions originate from different processes. Thus, we write

$$(12) \quad S_\nu = \frac{S_s + r_\nu S_c}{1 + r_\nu} = \frac{u_\nu S_s + S_c}{1 + u_\nu}.$$

We see that  $S_\nu$  can be treated as  $\nu$ -independent in only three cases: (i) when  $S_s = S_c \neq f(\nu)$ ; (ii) when  $r_\nu \ll 1$  and  $S_s \neq f(\nu)$ ; (iii) when  $r_\nu \gg 1$ , and  $S_c \neq f(\nu)$ . There is the fourth case that  $S_s$  and  $S_c$  each depend upon  $\nu$ , but in such a way that their variation combined with that of  $r_\nu$  leaves  $S_\nu$  independent of  $\nu$ . As a general possibility, this last seems too fortuitous to consider, when combined with the following remarks on the  $\nu$ -dependence of  $S_s$  and  $S_c$ .

Case (i) corresponds to LTE in both  $S_s$  and  $S_c$ , for  $S_s$  and  $S_c$  refer to different processes and can only coincide in the degenerate case. The results have already been discussed in Section 3'1.

Case (ii) corresponds to the core of a strong line, and the condition that  $S_s$  is  $\nu$ -independent over such a core. Since  $r_\nu$  increases monotonically outward

from the line-center, there must come some region on the line-profile where  $r_\nu S_\nu$  is not negligible compared with  $S_s$ . In this region,  $S_\nu$  varies with  $\nu$  because of  $r_\nu$ , even though  $S_s$  and  $S_c$  may be independent of  $\nu$ . Thus, there is at most a limited region of the profile which may be treated by a  $\nu$ -independent  $S_\nu$ . Whether even this limited region exists, must be shown by asking the form of  $S_\nu$ .

Case (iii) corresponds to a very weak line and the wings of stronger lines, and would appear to be the only case outside strict LTE where  $S_\nu$  remains  $\nu$ -independent over the whole profile. The variation of  $S_c$  with  $\nu$  over the very small width of the line can almost certainly be ignored. Actually, the situation is not so straightforward; for we must retain some quantity referring to the line in either  $S_\nu$  or  $\tau_\nu$  in order to produce a line. We return to this case under the designation of « weak-line approximation » below.

Consider the expectation on  $S_c$  and  $S_s$ .

In general, it appears sufficient to set  $S_c = B_\nu(T_e)$ —i.e. to assume LTE for the continuum—for discussions of most lines observed in the solar Fraunhofer spectrum. The point is the following. In the lower solar atmospheric regions, where  $r_\nu$  is not negligible for the lines formed in such regions, our present knowledge suggests that  $S_c$  does not depart appreciably from  $B_\nu(T_e)$  (cf. PAGEL, 1959; THOMAS and ATHAY, 1961). In the upper atmospheric regions, we must expect  $S_c$  to depart from  $B_\nu(T_e)$ ; for example, the Lyman continuum of hydrogen shows an  $S_c$  very different from  $B_\nu(T_e)$  (cf. THOMAS, 1952 and THOMAS and ATHAY, 1961, relative to the often-expressed, but incorrect, viewpoint contained e.g. in WOOLLEY and ALLEN, 1950). However, in these regions  $r_\nu$  can be shown to be so small that the value of  $S_c$  is not very important in the Doppler core, where the velocity field is important. (Note that the non-LTE effect drops  $S_c$  below  $B_\nu(T_e)$ , further reducing the relative importance of  $r_\nu S_{c*}$ .)

In the case of hot stars, and the outer solar atmosphere,  $S_c$  may depart from  $B_\nu(T_e)$  for two reasons. On the one hand, electron scattering plays the major role in the continuous opacity for hot stars. On the other hand, whenever the bound-free opacity arises in a region where occupation numbers differ from a Boltzmann distribution,  $S_c$  departs from  $B_\nu(T_e)$ . (For example, in the hydrogen Lyman continuum, as already mentioned.) These conclusions on  $S_c$  have two immediate consequences for our discussions of velocity fields. First, in the deeper atmospheric regions, one can use observations made in the continuum to fix  $T_e$ , hence the thermal velocity field. Then we may use the analysis of those lines whose « effective » depth of formation lies in the range covered by the data from the continuum to provide two kinds of wholly empirical check. On the one hand, one may infer a value of  $S_\nu$ , and compare



it with  $B_\nu(T_e)$ , to check the applicability of the LTE assumption. Such an analysis has been initiated by PECKER and associates (1959) and leads them to the conclusion that  $S_\nu \neq B_\nu(T_e)$  for a large number of weak lines of Ti, Ti<sup>+</sup>, Fe, V, Cr and Al<sup>+</sup>. On the other hand, one may assume a  $\nu$ -independent  $S_\nu$ , and analyse the profile as outlined in Sect 3'1 to infer a velocity field. If the field agrees with the thermal value, it is tempting to infer both that the  $\nu$ -independence assumption is correct, and that the only velocity fields existing are thermal. In the event that the velocity fields derived do not agree with the thermal value, one can either question the assumption on  $S_\nu$  or ascribe the discrepancy to a non-thermal velocity field. There exist a variety of analyses and results on this last procedure, which will be reported in detail in the various Parts of the program at Varenna.

Second, in hot stars and in the upper atmospheric regions where data from the continuum do not exist, the analysis of the lines must be used to fix both thermal and non-thermal velocity fields. Thus, considerable attention must be paid to the form of  $S_\nu$ . We mention the single exception, a limited region of the lower solar chromosphere, where it appears that eclipse observations made in the continuum provide independent data on  $T_e$  (cf. THOMAS and ATHAY, 1961). Throughout the outer stellar atmospheres, however, and over most of the outer solar atmosphere, both thermal and non-thermal velocity fields must be determined from analysis of the line-profiles alone. While a consistency requirement can be placed on thermal fields inferred from different lines of different ions—provided the relative regions of origin of the lines can be identified—the same consistency from ion to ion cannot be an *a priori* requirement on non-thermal velocity fields (for example, a superposition of gyromagnetic and turbulent motions).

When one turns to theoretical expectations on  $S_\nu$ , he must distinguish two kinds of treatment existing in the astrophysical literature. One is a kind of « working » approach to the analysis of spectral lines, based on formal rather than detailed physical analysis of the process of line-formation, which was developed mainly for discussion of total absorption in a line rather than of details in the line-profile. The other is a very specific attempt to treat in great detail the problems of line-formation from the single requirement that the observed spectrum does not change in time, thus, that the occupation numbers of internal energy levels of the various ions be constant in time.

The « working » approach characterizes by far the greatest bulk of existing astrophysical analyses of stellar spectra. The adopted expression for  $S_\nu$  follows from the process of simply writing down several possible mechanisms of producing radiation—which are taken to be « coherent scattering », « non-coherent scattering », and « pure absorption »—then assuming  $S_\nu$  is a linear combination of these alternatives, with coefficients whose numerical values are not specified a priori in terms of either atomic constants or thermodynamic

parameters of the atmosphere, but are to be fixed by the analysis. (Coherent and non-coherent scattering refer to frequency, not phase;  $S_s$  for pure absorption is  $B_\nu(T_e)$ , cf. UNSOLD, 1955, for a detailed summary.) With the exception of very strong Fraunhofer lines, the «working approach» is again simplified, in the great majority of analyses, by assuming that only the pure absorption term is significant—or that non-coherent scattering prevails in some strong lines and gives  $S_s = k B_\nu(T_e)$  ( $k$  being a frequency-independent constant, usually smaller than 1 for absorption lines). So long as either of the latter alternatives is valid, we have  $S_s$  independent of  $\nu$ , case (i) above, or case (ii), and the analysis is straightforward as discussed in Section 3'1. If the more general alternative including all terms mentioned were valid, the presence of the coherent-scattering term makes  $S_s$ , hence  $S_\nu$ ,  $\nu$ -dependent. For reasons discussed below, we reject this general alternative in the central parts of the line, where the absorption coefficient is mainly fixed by the velocity field. Thus, returning to the two points developed in Section 3'1 and summarized in Section 3'2, our concern with the form of  $S_\nu$  reduces to just one point, that of the degree to which the local value of  $S_\nu$  depends upon the local value of  $T_e$ . That is, the question of how literally the results of most of the existing astrophysical analyses can be taken, in discussing stellar atmospheric velocity fields, rests on how satisfactory is the assumption of LTE.

Investigations of the general form of  $S_s$  to be expected on the basis of the treatment of a gas in a statistically-steady, but not necessarily LTE, state have been motivated primarily by just this question of how significant are departures from LTE. General results from such investigations are presently few in number. There have been a number of detailed, numerical «brute-force» calculations, aimed mainly at producing results which can be compared with solar observations to see if details which are anomalous under the LTE approach become resolved under the non-LTE approach (cf. THOMAS and ATHAY, 1961, for a summary). Mainly, these calculations have been limited to hydrogen helium, and calcium. A sequence of algebraic investigations of simulated atoms, having a limited number of energy levels, has been initiated by JEFFERIES and THOMAS (1957, 1958, *et seq.*) in order to make more clearly explicit the thermodynamic parameters upon which  $S_s$  depends, and to link this more modern work with similar attempts in the early 1930's at more detailed investigation. We emphasize the significance of this older work: it was carried out under the conceptual limitations of no local energy dissipation other than radiative in the atmosphere. Thus, two points were missed: the possibility of an underestimate of the importance of collisional terms, because of the possible existence of regions where  $T_e$  exceeds the value inferred from the continuous spectrum alone; an underestimate of the height of formation of a spectral line relative to that of the continuum, again resulting from the greater value of  $T_e$ . On the other hand, much of the contemporary feeling that

these more modern non-LTE effects are confined to the stellar chromosphere overlooks the kind of non-LTE effects implicit in the older work.

The approximate results obtained from this sequence of algebraic investigations are most likely to be applicable to actual atoms where one treats strong resonance lines, or strong subordinate lines in atmospheric regions having very high opacity in the resonance lines. Work on a general methodology to extend the treatment to weaker lines is promising and suggestive, but only so, at this stage of development. The empirical work already cited, by PECKER and associates, suggests that the *general* physical results on the direction of departure from LTE may remain valid for weaker lines. We will now summarize briefly the results from these somewhat-idealized algebraic investigations, which indicate the extent to which  $S_s$  is  $\nu$ -independent, and to which the local values of  $S_s$  may be considered to depend only upon local parameters, particularly  $T_e$ .

One can write, quite generally,

$$(13) \quad S_s = B_\nu(T_{\text{ex}})j_\nu/\varphi_\nu,$$

where  $j_\nu$  and  $\varphi_\nu$  represent the profiles of emission and absorption coefficients, normalized such that their integrals over  $\nu$  (and solid angle, for  $j_\nu$ ) are unity.  $T_{\text{ex}}$  is the «excitation-temperature» defined as a Boltzmann temperature-parameter giving the actual ratio of occupation numbers in upper and lower levels of the transition. A solution of the equations of statistically-steady state for the occupation numbers, ignoring mass diffusion terms, gives (cf. JEFFERIES and THOMAS, 1958 *et seq.*; THOMAS and ATHAY, 1961)

$$(14) \quad B_\nu(T_{\text{ex}}) = \frac{\int \bar{I}_\nu \varphi_\nu d\nu + \varepsilon B_\nu(T_e) + \eta B^*}{1 + \varepsilon + \eta},$$

$\varepsilon$  is the ratio of rates of collisional to radiative de-excitation in the line, evaluated at the local value of  $T_e$  and  $n_e$ ; the term  $\eta B^*$  represents a ratio of upward excitations by radiative ionizations to spontaneous transitions downward in the line. Generally, in the stellar atmosphere, and for strong lines to which this two-level approximation has some degree of applicability, the first term on the right of eq. (14) is very much larger than either of the second two terms. In this event, the solution of the radiative transfer equation, using eq. (12), (13) and (14), is a diffusion problem, with the second two terms on the right of eq. (14) serving as «source» terms for the diffusion. With these expressions (13) and (14), consider the two questions we have raised: (i) the relation between the local value of  $S_s$  and the local values of the thermodynamic parameters characterizing the atmosphere; (ii) the relation between the  $\nu$ -dependence of  $S_\nu$ , that of the absorption coefficient, and that of  $I_\nu$  (emergent).



First, from a purely formal standpoint it is clear that only in two cases will  $B_r(T_{ex}) = B_r(T_e)$ , and thus will the local value of  $B_r(T_{ex})$  be fixed wholly by the local value of  $T_e$ .

a) If everywhere the first term on the right of eq. (14) is completely dominant, and satisfies  $\int \bar{I}_\nu q_\nu d\nu = B_r(T_e)$ . Such a condition could at most hold under highly exceptional circumstances, and certainly not throughout the atmosphere and for all lines.

b) If the second term on the right of eq. (14) dominates completely. For resonance and strong subordinate lines,  $\epsilon \ll 1$  in stellar atmospheric situation; so this possibility is excluded. Uncertainty on cross-sections for higher-lying subordinate lines couple with the uncertain applicability of the expression (14) to leave the situation unresolved. It would seem plausible that there exist pairs of energy levels close enough to the continuum that this second term dominates; the problem is to specify them, and for this we require cross-sections and more detailed treatments of the statistically-steady-state.

If neither of these two cases hold—and it is clear that neither will for the stronger lines in the stellar spectrum—then the local value of  $T_{ex}$  is not fixed by the local values of the thermodynamic parameters characterizing the atmosphere, but by their depth distribution.

Second, since opacity of the atmosphere to the continuous radiation is generally several orders of magnitude smaller than to line radiation involving the same lower level, there is a strong difference in result according to which is the larger of the second or third terms on the right of eq. (14). If the second term predominates over the third, then indeed the *distribution* of  $T_e$  fixes the values of  $S_\nu$ ; so that from an analysis of  $S_\nu(\tau_\nu)$ , we have a possibility of inferring  $T_e(\tau_\nu)$ . We must, however, treat the atmosphere as a whole. Examples of lines for which the second term in eq. (13) predominates over the first — which category we have called collision-dominated — are the  $H$  and  $K$  lines of ionized calcium, the  $Mg^+$  lines near  $\lambda 2700$ —and generally, the ionized metallic resonance lines—and the Lyman lines of hydrogen in the chromosphere. If, on the other hand, the third term predominates, then the source-term in the diffusion problem is simply the radiation field in the ionization continuum associated with the lower level of the line. This continuum originates in a much deeper atmospheric region than that where the line originates. Thus, if this continuum can be represented in terms of some temperature value, the value must be expected to differ very considerably from the local values of  $T_e$  in the region of line-formation. Examples of lines falling in this last category — which we have called photoionization-dominated — are the early Balmer lines of hydrogen, the sodium  $D$  lines and generally, the neutral metals.

Third, the value of  $S_s$  is independent of  $\nu$  at a given atmospheric position *only* if  $j_\nu/q_\nu$  is  $\nu$ -independent. It has been shown (THOMAS, 1957) that this latter condition is satisfied over that part of the line in which the profile of  $q_\nu$  is fixed by thermal motion. (If there exists a non-thermal motion, random over a scale much less than a photon free path, with mean velocity exceeding the thermal velocity for the atom in question, this same conclusion should apply to the larger line-core specified by this non-thermal velocity.) The behavior of  $S_s$  outside this central core—which is generally between 2 and 3 Doppler widths in size—has not yet been explored with conclusive results.

Fourth, since the source-terms in eq. (14) contain atomic parameters characteristic of the particular energy levels involved in producing the line studied, one must generally expect  $T_{\text{ex}}$  to differ from line to line, even in those cases where the lines may have one level in common. Exceptions may occur; these must be established by detailed investigation in each case.

Summarizing these results on  $S_\nu$  as they bear on the problem of the analysis of line-profiles for velocity fields, as outlined in Section 3'1, we can say the following:

$\alpha$ ) For strong resonance lines, or strong subordinate lines where lower-lying lines satisfy detailed balance;  $T_{\text{ex}} \neq T_e$ ;  $S_s$  is  $\nu$ -independent over the core of the line where the profile of  $q_\nu$  is essentially determined by the random velocity fields present;  $S_\nu$  is  $\nu$ -independent over the same core *if* over the same region  $r_\nu \ll 1$ ; in general,  $S_\nu$  differs from one line to another.

$\beta$ ) For weaker lines, and higher-lying subordinate lines, we have at present essentially no sound theoretical guide. On the one hand, we expect that resonance lines approximately described by eq. (14) will have  $T_{\text{ex}} \neq T_e$ ; but if they do not have  $r_\nu \gg 1$ ,  $S_\nu$  will depend upon  $\nu$ . On the other hand, for transitions between sufficiently-high-lying levels, we may expect  $S_s \rightarrow B_\nu(T_e)$ . No work has yet established the transition region. The empirical results by PECKER and associates suggest that this last regime of LTE has *not* been reached in the case of many lines for which it is usually assumed.

3'2.2. Application. In the following, we restrict our attention to absorption lines, from which most astrophysical information on velocity fields has come. There are exceptions, such as the discussion of very broad emission features in Wolf-Rayet stars and in novae, which ultimately must lead to very important information on velocity fields in atmospheres thought to be unstable in one way or another. Cf. the pioneering work by BEALS (1941), the extensive discussion by SOBOLEV (1947), such recent summaries as that by PAGEL (1959), and a short critique from the non-LTE viewpoint by THOMAS (1949). But by far the greatest body of information, upon which most astrophysical thinking is based, deals with absorption lines.

It is usually customary, in discussing absorption lines, to work with the depth of the line,  $R_\nu = I_c - I_\nu$ , rather than the residual intensity,  $I_\nu$ , in the line. From eq. (1) and (2) we have

$$(15) \quad R_\nu = \int_0^\infty \{S_c(1 - \exp[-\tau_s/\mu]) - \eta_\nu S_s \exp[-\tau_s/\mu]\} \exp[-\tau_c/\mu] d\tau_c/\mu.$$

It is also customary to express the above in terms of the «weighting-function»,  $g(\tau_c)$ , introduced for weak lines by UNSÖLD (1932), MINNAERT (1948), and extended to stronger lines by PECKER (1951):

$$(16) \quad \mu g(\tau_c) I_c = \int_{\tau_c}^\infty S_c \exp[-\tau_c/\mu] d\tau_c/\mu - S_s \exp[-\tau_c/\mu],$$

in terms of which, eq. (15) becomes

$$(17) \quad R_\nu = \int_0^\infty \eta_\nu (\mu I_c g(\tau_c)) \exp[-\tau_s/\mu] d\tau_c/\mu.$$

The equivalent width of the line is then given—converting  $I$  and  $R$  to wave-length rather than frequency units—by

$$(18) \quad W_\lambda = \int_0^\infty R_\lambda I_c^{-1} d\lambda.$$

The weighting-function approach is *mainly* used under the LTE assumption on  $S_s$ . In this case, the integrand in eq. (17) is a product of two factors, one— $\eta_\nu \exp[-\tau_s/\mu]$ —involving the line and depending upon  $\nu$ , and the other—the weighting-function  $g(\tau_c)$ —independent of the line and  $\nu$ . The latter, in the LTE case, is essentially the gradient of  $B_\nu(T_e)$ , and can be computed once and for all for a given atmospheric model. PECKER (1957) has emphasized that in the non-LTE case, the weighting-function,  $g(\tau)$ , can be written

$$(19) \quad \mu I_c g(\tau) = \left\{ \int_{\tau_c}^\infty S_c \exp[-\tau_c/\mu] d\tau_c/\mu - S_c \exp[-\tau_c] + (S_c - S_s) \exp[-\tau_c/\mu] \right\},$$

which becomes, in the linear case of eq. (9) for  $S_c$ ,

$$(20) \quad \mu I_c g(\tau) = \{\mu dS_c/d\tau_c + (S_c - S_s)\} \exp[-\tau_c/\mu].$$



Thus, for non-LTE effects to introduce significant change into the results of analysis of a line-profile,  $(S_c - S_s)/S_c$  need not be of order unity, but only of order  $\mu \, d \ln S_c / d \tau_c$ . We also note that in the general non-LTE case, where the scattering term,  $\int I_r g_r d\tau_r$ , entering eq. (14), is of major importance, it is quite misleading to retain the usual physical picture of the weighting-function as something characteristic of the model of the atmosphere and independent of the particular line considered. The scattering term often depends upon  $\tau_c$  almost independently of  $\tau_s$ , particularly for strong lines. Thus, the utility of  $g(\tau)$  as a function that can be computed once and for all, independently of the line, largely disappears when non-LTE effects must be included. It is, however, a very useful concept to demonstrate, as in eq. (20), the quantities with which non-LTE effects must be compared to assess their importance.

If we consider the case where  $S_s$  is independent of  $\nu$ , then we may regard  $g(\tau)$  as that part of the integrand which is independent of  $\nu$ , the  $\nu$ -variation coming from the factors  $\eta_\nu$  and  $\exp[-\tau_s/\mu]$ . The first factor alone would give something resembling the laboratory case of a thin atmosphere, since it is just the profile of the absorption coefficient. The second factor selects the atmospheric region contributing most to the particular point on the profile thus giving the contribution to the profile arising from the variation in  $g$  with  $\tau$ —through the variation in the  $S$  with  $\tau$ . Thus, in astrophysics it is customary to distinguish two kinds of lines: «weak» lines, for which  $\tau_s \sim 0$  is a good approximation; and all other lines, for which non-zero  $\tau_s$  must be considered. We consider those two kinds of lines, in turn.

a) The weak-line approximation. The basic assumption is  $\tau_s/\mu \ll 1$ , so that  $\exp[-\tau_s/\mu]$  may be taken as unity. It is often assumed, in studying such weak lines, that the profile of  $R_\nu$  mimics the profile of the absorption coefficient, through the  $\nu$ -dependence of  $\eta_\nu$  (Cf. BELL, 1951; BELL and MELTZER, 1958; ROGERSON, 1957). If the assumption were valid, we should have the astrophysical analogue of the case of a uniform, thin gas in the laboratory, and the observed line-profile would give immediately the velocity field. We see that this assumption requires a constant profile of  $q_\nu$  over whatever region of the atmosphere contributes significantly to the line. Such a constant profile of  $q_\nu$  is often justified by the argument that the line is formed in a narrow region of the atmosphere (the so-called Schuster-Schwarzschild case), because of strong variation in excitation conditions. Thus, in applying this weak-line approximation to study the distribution of velocity fields in the atmosphere, it is important to distinguish the case just cited from that where the ion is distributed more-or-less uniformly over the region where the continuum originates (the so-called Milne-Eddington case), but the ion considered has a very low abundance.

We have already mentioned the second assumption, that  $S_s$  must be

$\nu$ -independent, if  $\eta_\nu$  is to give the whole  $\nu$ -dependence. This second assumption is invariably overlooked, because the analyses invariably assume LTE. However, its neglect is not serious, in view of our proof, mentioned earlier, that  $S_s$  is  $\nu$ -independent over the Doppler core. The weak-line criterion of  $\tau_s \ll 1$  ensures that the line will have significant opacity only over this Doppler core.

Thus, a departure of the profile of  $R_\nu$  from a simple Doppler profile, that of  $q_\nu$ , comes only from a variation of  $q_\nu$  over the atmospheric region contributing to the line. Alternatively, such an observed departure may be taken as evidence that the line really does not satisfy the weak-line criterion. Finally, departure from agreement of profiles of several «weak-lines» originating from several ions either signifies a variation of conditions within the atmosphere, or varying velocity fields from ion to ion producing the lines.

The alternative among these effects must be considered carefully and seriously. An example lies in the suggested procedure to distinguish thermal from non-thermal velocity fields in the solar atmosphere by comparing profiles of  $R_\nu$  for weak lines from several ions of differing mass (BELL, *ibid*). HOUTGAST (1953) has stressed the difficulties entering such an analysis from the standpoint of differing distributions within the atmosphere of the ions considered. For several of her lines, Miss BELL has found it necessary to interpret the profiles with Doppler plus damping contributions to the absorption coefficient, which implies that the lines do not satisfy the weak-line approximation.

Finally, the criterion used to select lines satisfying the weak-line approximation is often not based on a computation of the validity of the condition  $\tau_s \ll 1$ , but only upon the observational criterion that  $R_\nu <$  some small fraction. Such a procedure essentially neglects the effect of the difference ( $S_\nu - S_s$ ), assuming this quantity to be zero. In a similar way, we note that  $\tau_\nu = 1$  does not imply that for all regions of the atmosphere,  $\eta_\nu \ll 1$ . A weak line may originate entirely within the chromosphere, where  $\tau_\nu$  is essentially zero, for example. Both these points warrant investigation.

b) Lines not satisfying the «weak-line» approximation. Since  $\eta_\nu$  is directly proportional to  $\alpha_\nu$ , whose integral over  $\nu$  is  $\pi e^2 f_{12} / mc$ , we see from eq. (17) and (18) that in the weak-line approximation,  $W_\lambda$  is independent of the velocity field. Thus, only the line-profile gives information on velocity fields. When observational conditions are such that only integrated intensities, or equivalent widths, not profiles, can be observed, the weak-line case gives no information on velocity fields. However, as  $\tau_\nu$  increases to the point where the weak-line approximation becomes invalid,  $W_\lambda$  depends upon the velocity field and we may use both profile and integrated intensity to study the velocity fields.

For illustration, consider the LTE case,  $S_s = S_c - B_\nu(T_e)$ , and an extreme case of the (Schuster-Schwarzschild) kind of model already mentioned, where the emitting ion is confined to a narrow atmospheric layer, at  $\tau_c$ , of negligible thickness in  $\tau_c$ . Then eq. (17) integrates to

$$(21) \quad R_\nu \sim \mu(1 - \exp[-\tau_s/\mu]) \exp[-\tau_c/\mu] dS_c/d\tau_c.$$

If we consider a sufficiently-strong line, we see that the line « saturates » in its central regions, maintaining practically a constant value of  $R_\nu$  until  $q_\nu$  decreases sufficiently to drop the value of the bracket in eq. (21) below unity. Thus, the integrated profile of  $R_\nu$ , or the equivalent width  $W_\lambda$ , depends strongly upon the parameters fixing the rate of drop of  $q_\nu$ . In a rough way, for wholly random velocity fields

$$(22) \quad q_\nu/q_0 \sim \exp[-[\Delta\nu/\Delta\nu_D]^2] + F([\Delta\nu]^{-2}),$$

so that the greatest rate of decrease in  $q_\nu$  comes over the core of the line.  $F$  relates to the radiation and collisional broadening processes. For weak lines,  $W_\lambda$  depends only upon  $\tau_{s_0}$  (subscript 0 referring to the line-center). As  $\tau_s$  increases  $W_\lambda$  begins to depend upon  $\Delta\lambda_D$ , until, for strong enough  $\tau_s$ ,  $W_\lambda$  is proportional to  $\Delta\lambda_D$ , and varies only slightly with  $\tau_{s_0}$ . When the line becomes so saturated that the second term on the right of eq. (22) becomes of major importance before the saturation begins to disappear, we enter the well-known « pressure-broadening » regime, and  $W_\lambda$  varies as  $\tau_{s_0}^{1/2}$ . A plot of this dependence of  $W_\lambda$  on  $\tau_{s_0}$ , with parameter  $\Delta\lambda_D$ , is called the curve of growth. Fig. 3, in our discussion of a rough approximate treatment of the total absorption by a line, represents a rough approximation to a curve of growth. For a detailed discussion free from the special assumptions underlying eq. (21), cf. the classical discussion by MENZEL (1939), and the recent summary by VAN REGEMORTER (1958).

Let us now summarize the classical theoretical computation of a curve of growth from the weighting-function approach. It is based on the use of the weighting-saturation functions (see, for instance, in the limited case of LTE for  $g(\tau_c)$ , UNSÖLD, Physik der Sternatmosphären, p. 109). The expression of  $W$  can then be written

$$(23) \quad W = \int_0^\infty \frac{R_\lambda}{I_c} d\lambda = k \int_0^\infty g(\tau_c) \frac{\eta_{\nu_0}}{\bar{\eta}_{\nu_0}} \Phi(\bar{\eta}_{\nu_0}, \alpha) \sqrt{T_e} d\tau_c,$$

$g(\tau_c)$  being the weighting-function, as defined in eq. (16),  $\Phi(\bar{\eta}_{\nu_0}, \alpha)$  (Fig. 8) being the saturation function ( $\Phi = 1$  if  $\bar{\eta}_{\nu_0} \ll 1$  — weak lines —, and  $\Phi < 1$



in the other cases—intermediate strength, and strong lines). The function  $\Phi$  has been extensively tabulated.  $\eta_{v_0}$  is the value of  $\eta_v$  at the center of the line; and one has

$$(24) \quad \bar{\eta}_{v_0} = \frac{1}{\tau_c} \int_0^{\tau_c} \eta_{v_0} d\tau_c.$$

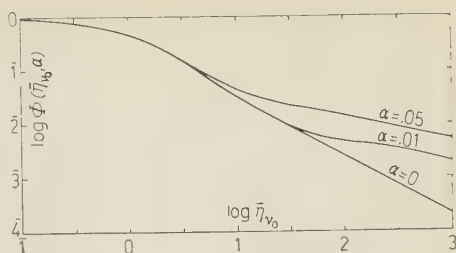


Fig. 8. — Saturation function.

One can write, in an approximate way, just introduced to show the respective importance of each of the physical factors involved,

$$(25) \quad W \sim k[g(\tau_c)\sqrt{T_e}] \int_0^{\infty} \frac{\eta_{v_0}}{\bar{\eta}_{v_0}} \Phi(\bar{\eta}_{v_0}, \alpha) d\tau_c.$$

The variation of  $\eta_{v_0}/\bar{\eta}_{v_0}$  with  $\tau_c$  fixes essentially (and only) the detailed shape of the curve of growth; the values of  $g(\tau_c)$  and of  $T_e$  fix the value of  $W$  corresponding to the plateau of the curve of growth (Fig. 9). Through this

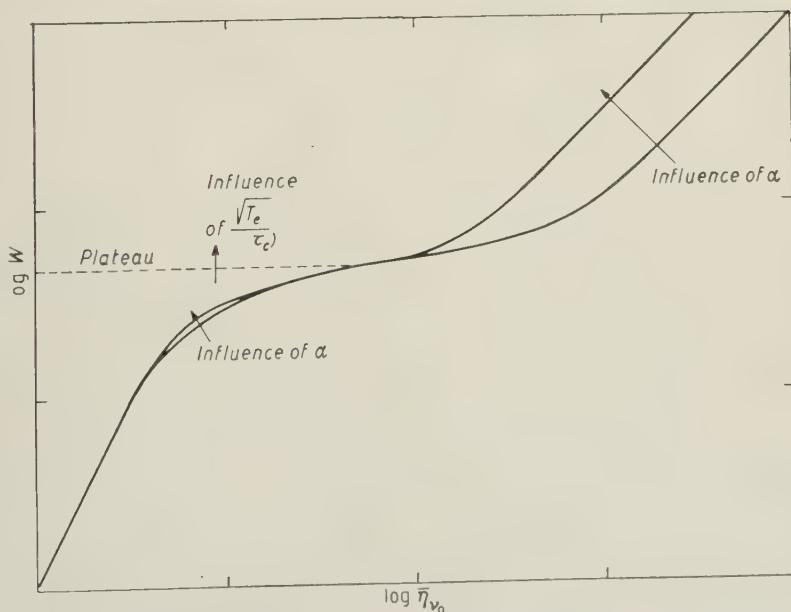


Fig. 9. — Influence of saturation function on plateau of curve of growth.

value, determined from measurements, and provided one knows the variation of  $g(\tau_c)$ , it is thus possible to derive a mean value of the thermal motions.

It must be noted that the above analysis assumes  $S_c$  and  $S_s$  to be not  $\nu$ -dependent (*i.e.*  $g(\tau_c)$  non  $\nu$ -dependent); the case with a  $\nu$ -dependent  $g(\tau_c)$  has never been treated in this approach: this limitation thus puts a great concern with this weighting-saturation approach. It has been treated, however, rigorously in limited cases (pure coherent scattering and approximations on  $\eta_{\nu_0}$  of the Milne-Eddington or Schuster-Schwarzschild type — see *e.g.* WRUBEL, 1949). But in those limited cases, the «pure-scattering» restriction on  $S_s$  and the overlooking of the actual stratification through the approximations on  $\eta_{\nu_0}$  put again a great doubt on the results that can be derived, through such curve of growth, about velocity fields.

We denote lines not strong enough to enter the «pressure-broadening» regime as «lines of intermediate strength»; others, as «strong lines». In the former, both profile and  $W_\lambda$  depend upon the velocity fields; in the latter,  $W_\lambda$  is most insensitive to the velocity field, and only the profile may be used.

Generally, in analysing a line for velocity fields, three effects must be considered: the distribution of  $q_\nu$  through the atmosphere; the effect of departure of  $S_s$  from  $S_c$ ; the effect of  $\nu$ -dependence of  $S_s$  outside the Doppler core. There exist no systematic investigations of the latter two effects. In discussing the problem of variable  $q_\nu$ , analyses reported thus far have simply made some assumption on variation of  $q_\nu$ , then compared results from lines thought to originate in differing atmospheric regions, to construct a better approximation (or, if center-limb observations exist, they may be used in place of several lines having differing heights of origin). So long as we restrict attention to atmospheres having only thermal velocity fields, as in this Section 3'2, and to those parts of the atmosphere where the *form* of the distribution in  $T_e$  is known, an a priori assumption on the kind of variation of  $q_\nu$  is feasible. Such a situation is possible for weak and intermediate strength lines. Because  $\eta_{\nu_0}$  is so large for strong lines, those regions of the line where velocity fields are important are often formed outside the atmospheric regions where the general distribution of  $T_e$  is known. In the more general case of Section 3'3 and 3'4, any a priori estimate of general distribution of velocity field is unsound, because of our present complete lack of theoretical knowledge of the kinds of velocity fields to be expected in a stellar atmosphere. The most one can ask, in any iterative procedure, is numerical consistency (*cf.*, *e.g.*, HUANG, 1951).

In summary, on the basis of presently-developed methods of analysis, the most important aspect in an a priori approach is that of assigning «effective» depths of formation for differing lines. The question of such effective depths has, until now at least, been investigated from the LTE basis (*cf.* VAN REGEMORTER, 1958). It is now well-established that non-LTE effects combine with the existence of a stellar chromosphere to introduce very serious change, over the classical LTE computations, in depth of formation of the Doppler cores of strong, and intermediate lines (*cf.* THOMAS and ATHAY, 1961,

for a summary of work leading to this conclusion). It is a problem to be settled, how far such effects extend out from the core of the line, and their influence on  $W_\lambda$ . Another problem is to make a clear distinction between what could be called « effective depths of formation of Doppler widths » and « effective depths of formation of central intensity » etc. In an exact treatment, no problem arises; but the iterative methods used are hardly exact. Every method of measurement corresponds to a different « effective depth » and the interrelations between them have not been satisfactorily analysed.

We would emphasize that, since there is no *a priori* knowledge whether an atmosphere satisfies the condition of wholly thermal velocities, it is critical that an analysis of the atmosphere provide a check of inferred velocity field against the thermal value. For those weak lines formed in regions covered by analysis of the continuum, an immediate check exists. For intermediate-strength lines, a comparison of profile to total absorption gives a check. (Cf. the discussion of micro- and macro-turbulence in Sections 3'3 and 3'4 on this point.) For strong lines, such a check is more difficult. We must require either a determination of  $T_e$  from the magnitude of  $S_\nu$ , then a determination of velocity field from absorption coefficient via the  $\nu$ -dependence of  $I_\nu$ ; or a determination of velocity field *at a given point in* the atmosphere from several lines of different ions, and intercomparison to see whether a wholly thermal origin is consistent. Thus, the question of non-LTE effects becomes of primary importance, in attempting to analyse observations of such strong lines for atmospheric velocity fields.

We have already differentiated, in the last few paragraphs of Sect. 3'2.1, between essentially two types of  $S_\nu$ —one of which depends upon collisional effects for the source term, thus upon the distribution of  $T_e$  through the region of line-formation; the other of which depends upon photo-excitation, thus is insensitive to  $T_e$  in the region of line formation. An analysis of a line of the first type may by itself provide a set of data with internal checks. An analysis of a line of the second type gives information on thermal velocity fields only from the  $\nu$ -dependence of  $I_\nu$ , and has no checks for consistency of the assumption that the velocity fields are wholly thermal. Thus, several lines of different ions must be analysed, whose Doppler cores are formed in the same atmospheric region. Locating the region of formation is a problem comparable to specifying the velocity field, and the two must be solved together.

3'3. *Analysis of a line formed in an atmosphere where non-thermal velocity fields exist, but are assumed to consist of random motions of groups of atoms of dimension smaller than a photon free-path.* — Clearly, the photon free-path in question (a length corresponding roughly to an optical depth unity) must be that corresponding to the largest value of the line absorption coefficient, that at the line center. Then, for a given ion, this motion is indistinguishable from



the thermal motion in its effect upon absorption coefficient and  $\nu$ -dependence of  $S_s$ . We simply write the resultant mean square velocity:

$$(26) \quad \overline{V_{\text{tot}}^2} = \overline{V_{\text{therm}}^2} + \overline{V_{\text{micro}}^2} = \frac{kT_e}{m_i} + V_{\text{micro}}^2.$$

We use the subscript «micro» in conformity with astronomical usage of the term «microturbulence» to describe this small-scale, non-thermal velocity component. The «microturbulence» differs from the thermal motion in two respects: it need (\*) not vary with the atomic mass,  $m_i$ ; it need not be isotropic.

The same type of analysis may, consequently, be used under this condition 3'3 as under condition 3'2. The difference is, that what was a check between several measures of thermal motion becomes now a comparison of  $V_{\text{therm}}$  and  $V_{\text{micro}}$ .

This intercomparison may be made between  $T_e$  determined from  $S_s$  and from the  $\nu$ -dependence of  $I_\nu$ . Or, it may be made between values of  $T_e$  inferred from either of these methods applied to ions of different mass (*e.g.* the investigations like those of Bell already cited in Section 3'2. If there is only a random component in the non-thermal motion, eq. (26) may be used to obtain  $V_{\text{therm}}$  and  $V_{\text{micro}}$ . An inferred difference between these several quantities may either be taken literally, or used as a basis to question the validity of the analytical methodology, from the standpoint of the uncertainties raised in Sections 3'1 and 3'2.

Several analyses of astrophysical data have produced results implying  $V_{\text{micro}} > V_{\text{sound}}$ , where  $V_{\text{sound}}$  is essentially  $V_{\text{therm}}$  for hydrogen, evaluated at what is assumed, in these analyses, to be  $T_e$  in the atmospheric region analysed (*cf.* STRUVE and ELVEY, 1934; UNSÖLD, 1929, WILSON and BAPPU, 1957). It has been objected that such results are physically inconsistent from a gas-dynamical standpoint (THOMAS, 1948) if the atmosphere is to be in a time-steady thermodynamic state—they would lead to a rapid mechanical energy dissipation and a rise in  $T_e$ . Therefore, either the assumed values of  $V_{\text{therm}}$  in the atmosphere are too low, or the analytical methodology underlying the results is incorrect.

Probably the most controversial aspect of results on «microturbulence», aside from the above results concerning supersonic microturbulent velocities, lies in the question of anisotropy *vs.* depth-dependence. These results come from study of weak and intermediate strength lines in the sun, where

(\*) Most authors assume that the microturbulence obviously does not depend upon atomic mass—but in the case of such motion as gyromagnetic, the velocity varies with  $m$ . Such possibilities must be clarified.

centerlimb data may be obtained. (Cf. ALLEN, 1949; RICHARDSON and SCHWARZSCHILD, 1950; HUANG, 1951; SUEMOTO, 1957; WADDELL, 1958; ROGERSON, 1959.) In essence, as one observes along the line of sight progressing from center to limb, he both observes at lesser effective depth and along a non-radial direction. The problem is to distinguish, in an inferred change in «microturbulent» velocity, between the depth variation and a possible anisotropy. An argument (cf. WADDELL) in favor of anisotropy comes from the fact that if no anisotropy is assumed, but all effects laid to a depth variation, the parameters describing such depth variation are found to depend heavily upon the line chosen. In our opinion, this discrepancy may also arise from differential non-LTE effects (cf. PECKER and VOGEL, 1960). Much more work needs to be done on these questions before we can consider that we have a clear-cut kinematical picture of the velocity fields actually existing. Again, detailed discussion is best deferred to the presentation of results in the following papers. Here, we only emphasize the point as an important one from the standpoint of the analytical methodology.

3'4. *Analysis of a line formed in an atmosphere where quite general macroscopic velocity fields are admitted.* — In essence, we have four kinds of velocity fields to consider. In addition to those already treated: (i) thermal and (ii) non-thermal but random over all dimensions larger than some scale much smaller than a photon free-path — we have: (iii) mass motion of some type other than (ii) but having no gradient horizontally, and (iv) horizontal gradients in the mass motion. If we had arbitrarily-good geometrical resolution, we could restrict our attention to types (i)-(iii), or motion in a narrow cylinder of gas. Uniform systematic motion of the cylinder does not alter any of the approach already discussed, the line is simply displaced as a whole. What requires to be discussed, is a vertical velocity gradient in the motions of type (iii). Lacking good geometric resolution, the effects of type (iv) broaden the line profile. For example, note the simple case of a collection of columns, within each of which the gas moves up or down as a whole.

Generally, in astrophysics, motion of type (ii) is called «microturbulence», and the term «macroturbulence» is rather loosely applied to a compound of (iii) and (iv). In formulating an analytical methodology, for discussing the effect of velocity fields upon spectral line profiles, it is important to distinguish carefully between types (iii) and (iv); such a distinction is more often blurred in the astrophysical literature than not. For example, HUANG and STRUVE developed their method of line-width *vs.* equivalent-width correlation in terms of a situation resting upon motions of type (i)-(iii), then applied it to situations which included the effect of type (iv). HUANG has kindly answered our inquiry on this point by stating that it was their intent that the method should be applied only in situations where the «macroscopic» types (iii) and (iv) do

not seriously alter the profiles obtained from «microscopic» motions of types (i) and (ii) alone. We could only emphasize that analyses and comparisons of results from different kinds of analyses (such as discussions of line-profiles *vs.* discussions of equivalent-widths) must be done with a very clear picture of the kind of motions assumed; since, for example, the differential effects of «microscopic» and «macroscopic» motions upon line-profiles and curve of growth are appreciable.

Little formal work has been done on this problem of interpreting line-profiles for generalized velocity fields, aside from that by HUANG and STRUVE just cited, mainly because of the observational difficulties cited, noting our earlier remarks that a good discrimination of non-LTE and velocity effects usually depends upon the analysis of intensity differences of several percent in the line-center. Modern photoelectric work with good gratings now begins to make such discrimination a possibility. So, we first summarize the Huang-Struve approach, then add a few comments from the standpoint of the developments summarized earlier in the present paper.

**3'4.1. The Huang-Struve approach.** They orient their discussion around a distinction between «physical Doppler broadening» and «geometrical Doppler broadening». The former represents whatever line-broadening would result from the velocity distribution within a column of gas lying below some surface element. The effect of superposing several columns of gas distributed over the surface of the star, they call geometrical broadening. This last is the observed quantity, which they write as

$$(27) \quad R(\lambda) = \int_{-\infty}^{\infty} B(\Delta\lambda) R'(\lambda - \Delta\lambda) d(\Delta\lambda).$$

The quantity  $R'(\lambda - \Delta\lambda)$  represents the «physical Doppler broadening», thus, an integral over depth, and  $B(\Delta\lambda)$  represents the geometric integration effect—limb-darkening, variation in systematic mass-motion of columns, stellar rotation, etc. Thus, the basic assumption is that the quantity  $R'$  can be determined for an atmospheric model having no «geometrical» broadening. One then introduces various broadening functions,  $B(\Delta\lambda)$ , and attempts to match the observed profiles,  $R(\lambda)$ . (Cf. HUANG and STRUVE, 1953, for a discussion of various broadening functions, and the difficulty of distinguishing between these functions for several types of mass motion.)

In the situations that either the effect of  $B(\Delta\lambda)$  greatly predominates over that of  $R'$ , or conversely, the resultant  $R$  is essentially the predominant one of the two quantities. When  $B$  and  $R'$  are comparable in their effect, HUANG and STRUVE have attempted to separate their effects by studying the relation



between equivalent width, half-width, and central intensity of the line (1952*a*, 1952*b*, 1955). In the first two papers, the methodology developed rests upon the implicit—and somewhat paradoxical—assumption that the macroscopic motion does not seriously alter the profile obtained from considering only microscopic motions; the third paper attempts to remove this limitation. Huang and Struve recognize the uncertainty introduced by uncertainty on the theory for the central intensity of the line (which we would re-emphasize on the basis of our discussion of  $S_s$ ). All we can really say, is that the problem remains to be investigated from the standpoint of a complete theory of  $S_s$  and its effect.

These discussions by Huang and Struve direct their attention to the very practical problem of analysing the observed stellar spectra. The solar case, with its resolution of the disk, provides an easier problem. Therefore, we conclude with a summary of the methodology in the case of two simple types of motion within «columns» that can be resolved. We deal, then, with the problem of specifying the  $R'$  function of Huang and Struve.

**3.4.2. Vertical gradient in mass-motion; only thermal random motions.** In essence, the presence of a gradient in systematic motions exhibits itself as an asymmetry in line-profile. For illustration, continue the assumption that  $S_\nu$  is  $\nu$ -independent, and restrict attention to the Doppler core of the line. Then we have

$$(28) \quad \varphi_\nu = \varphi_0 \exp \left[ - \frac{[\nu - \nu_0(1 + V(\tau)/c)]^2}{(\Delta\nu_D)^2} \right].$$

Again for illustration, adopt a caricature version of the result of the linear relation of eq. (7); viz., assume that rigorously

$$(29) \quad I_\nu(0, \mu) = S_\nu(\tau_\nu = \mu).$$

Then if  $V(\tau)$  has everywhere the same sign (*i.e.* always the motion is up or down), we see that the points to which we «see» at equal distance from the center of the line differ. That is, picking points of equal  $I_\nu(0, \mu)$  on opposite sides of the line-center, we do not go equal distances ( $\Delta\nu$ ) from the line-center. If we label these points of equal intensity by  $\nu^+$  and  $\nu^-$ , and if  $V$  were constant at all heights above that corresponding to  $I_\nu$ , we would determine  $V$  from points of equal  $\varphi_\nu$  in eq. (28) as

$$(30) \quad V/c = [(\nu^+ - \nu_0) - (\nu_0 - \nu^-)]/2\nu_0.$$

Since  $V(\tau)$  is not necessarily constant, one must proceed by successive approximation, but the principle is the same. Indeed, it is very similar to that discussed in Section 3'1 for determining  $\Delta\nu_D$ . Were these simplified

assumptions satisfied, there would be no serious problem in determining  $V(\tau)$  (cf. Fig. 10).

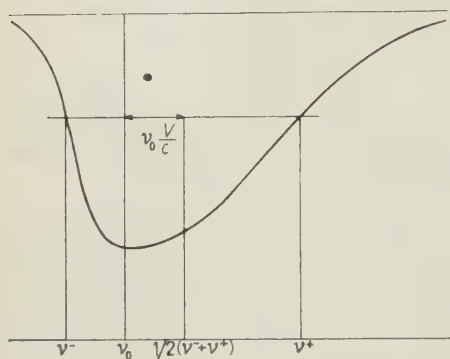


Fig. 10. — Vertical velocity gradient; eq. (3C).

There are two kinds of difficulties associated with departure from the simplified assumptions of the last paragraph. First, there is the problem of inverting the integration of  $S_\nu$  over  $\tau_\nu$ , to replace the simplified relation (29). The most direct approach is to investigate line-profiles via models of  $S_\nu(\tau_\nu)$ . For the LTE assumption, the procedure is fairly straightforward; for one uses the same function  $T_e(\tau_c)$  for all lines.

Then it is a question of investigating  $V(\tau_c)$ . The second problem is the more serious—if one does not assume LTE, what kind of function  $S_\nu(\tau)$  is to be used in proceeding, even by the method of models?

One procedure is to attempt to duplicate the procedure sketched in Section 3'1 for an empirical determination of  $S_\nu(\tau)$ , based upon the assumption that several lines having the same  $S_\nu$  can be found. It must first be shown that there are such lines. Second, since there are now two unknown functions to determine— $\Delta\nu_D(\tau)$ ,  $V(\tau)$ —more than two lines must be found satisfying the condition. Or, center-limb variations must be used, in the manner outlined in Section 3'1.

Thus, in any event, the problem comes down to discussing the question of  $S_s$  in an atmosphere with macroscopic velocity fields. We have already sketched the existing theoretical approach to  $S_s$ , in an atmosphere having only thermal motions, centered around eq. (13) and (14) as a first approximation. Consider quickly the modifications introduced by the presence of macroscopic velocity fields.

The formal procedure associated with the eq. (12) and (14) lies in investigating  $S_s(\tau_0)$  in an atmosphere having some theoretically-prescribed distributions  $T_e(\tau_0)$ ,  $n_e(\tau_0)$ . Then, one solves the equation of radiative transfer, using eq. (12) and (14) and these distributions. Thus, the changes in  $S_s(\tau_0)$  which might be expected to occur in an atmosphere having macroscopic velocity fields as compared with one having thermal motions only are of two types.

First, there may occur a significant change in the  $T_e(\tau_0)$ ,  $n_e(\tau_0)$  which we would prescribe from wholly theoretical considerations, this change resulting

from the added energy input from mechanical dissipation of the macroscopic velocity field. In addition to changing the details of the  $S_s(\tau_0)$  distribution, this change in  $T_e(\tau_0)$  may well change the *type* of  $S_s$  for a given line from the photoionization-dominated type ( $S_s$  largely independent of  $T_e$ ) to the collision-dominated type ( $S_s$  dependent upon  $T_e(\tau_0)$ ). Fixing  $n_e$ ,  $T_e$  from a wholly empirical determination eliminates this problem.

Second, the presence of the macroscopic velocity field alters the opacity within the line, as given by eq. (30). To see the effect, we digress briefly on the method of solution of the transfer equation, using eq. (12) and (13). (Cf. JEFFERIES and THOMAS, *ibid.*) Since the resulting equation of transfer is an integro-differential equation, some method of algebraic quadrature must be applied to the integral over  $\nu$  in eq. (14). In the case of wholly-thermal motions,  $I_\nu$  is symmetric about the line-center. Further,  $q_\nu$  falls off so rapidly with increasing  $\Delta\nu$  that the investigations thus far completed have assumed it sufficient to treat only the Doppler core. The quadrature is then straightforward. The asymmetry introduced by the macroscopic velocity field, however, requires separate treatment of the two sides of the line, thus doubling the number of quadrature points, and introducing a more-elaborate depth dependence of  $q_\nu$ . The problem has not been touched to date.

**3.4.3.** All macroscopic motions random over a scale larger than some dimension smaller than a photon free-path. In thinking through the analytical approach to an analysis of the velocity field, we have essentially two alternative conceptual points: *a*)  $S_s$  has some given geometrical distribution, not dependent upon the particular line analysed—*e.g.*, LTE; *b*) the distribution of  $S_s$  is a strong function of  $\tau_0$ , the opacity in the center of the line, and possibly the distribution of opacity about the line center.

*a*)  $S_s$  has a given geometrical distribution, not dependent upon properties of the line analysed.

Here we recognize that the essential quantity fixing  $I_\nu$  is how geometrically-deep in the atmosphere do we see it at a given  $\Delta\nu$  from the line-center. That is, how geometrically-deep must we go before encountering along the line of sight enough atoms having velocity  $V$ , where

$$(31) \quad \Delta\nu = v_0 V/c,$$

to build up  $\tau(\Delta\nu) \sim 1$ . If we forget for the moment natural and collisional broadening of a line, we then see that in discussing line formation under this case *a*), the velocity distribution function of direct interest is not at a given point in the atmosphere. Rather, we want the function  $\tau_c(V)$ , which is the distance down into the atmosphere one must go before encountering  $N$  atoms,



in the lower level of the line considered, having velocity  $V$  along the line of sight. Here,  $N$  is the same for all  $V$ , simply being given by

$$(32) \quad \frac{\pi e^2}{m c} f_{12} N \sim 1.$$

Clearly,  $\tau_c(V)$  results from the integration of the distribution functions at a point, but these last are not the quantities of direct interest, nor are they obviously the easiest in which to formulate a description of random motions of varying scale. Given  $\tau_c(V)$ , we immediately have  $\tau_r(V)$  from eq. (31) and the (assumed known) value of abundance of ion in question to the source of continuous opacity. Thus, we may integrate eq. (1) and obtain  $I_\nu$ . Conversely, if we know  $S_s(\tau_c)$ , we may invert an observed  $I_\nu$  to obtain  $\tau_c(V)$ .

The results from this inversion do not give the velocity distribution function at a point. To obtain this, one must analyse several lines, having different  $f_{12}$  values, then take the difference of the derived  $\tau_c(V)$ .

The actual presence of natural and collisional broadening must be included. To compute the relevant collision rates, we require an average over the local velocity distribution function. Since this last is *a priori* unknown, one must derive it as in the last paragraph, then iterate the procedure.

*b)*  $S_s$  depends upon  $\tau_0$  for the line considered, and possibly upon the distribution of opacity about the line-center. We return again to this question of the influence of velocity fields on the derived  $S_s$ . We have already commented on the two aspects changing the distribution derived for a quiescent atmosphere—a possible rise in  $T_e$  because of aerodynamic dissipation effects, and a change in opacity above a given geometrical point. There is no need to belabour the point, particularly since no work has been done on it.

Consider an extreme example, a column of gas consisting of two parts, one lying above the other, in relative motion at a speed large with respect to the internal thermal motion—we suppose there is no other motion. Now a photoionization-controlled  $S_s$  for a very strong line varies, over the central core, only with  $\tau_0$ . Therefore, let each part of the column have thickness  $\tau_0 \geq 10^4$ , but not so great as to be opaque in the ionization continuum of the transitions considered. Then, if we consider the relative speed to correspond to, say, ten Doppler half-widths, the cores of the resulting two lines will be well-separated, and have the same profile, and intensity. As the relative speed decreases, the cores begin to merge, and the distributions  $S_\nu(\tau_0)$  begin to be fixed by the conditions in the two parts of the column together, rather than there being two distinct parts of the column and two distinct line cores. The point which we would make here, is simply that a discussion of micro- and macroturbulence, in the usual sense, applied to a single column of gas requires a detailed discussion of the form and behavior of  $S_s$ .

## BIBLIOGRAPHY

- ALLEN, C. W. 1949, *M. N.*, **109**, 173.
- ATHAY, R. G. and THOMAS, R. N. 1957, *Ap. J.*, **62**, 3; 1958, *Ap. J.*, **127**, 96.
- BEALS, C. S. 1941, *Colloq. Int. d'Ap. Actualités Sci. et Indust.*, **901**, 169, Paris.
- BELL, B. 1951, *Harvard Observatory*, Spec. Report No. 35.
- BELL, B. and MELTZER, A. 1959, *Smith Contr. Ap.*, **3**, No. 5, 39.
- CHANDRASEKHAR, S. 1934, *M. N.*, **94**, 16.
- DE JAGER, C. 1952, *Rech. Astr. Obs. Utrecht*, **13**, Part I, 50; 1959, *Hd. d. Phys.*, **42**, 80, Berlin.
- GOLDBERG, L. 1958, *Ap. J.*, **127**, 308.
- GOLDBERG, L., MOHLER, O. C. and MUELLER, E. A., 1959, *Ap. J.*, **129**, 119.
- HOUTGAST, J. 1953, *Proc. Conv. Volta* p. 33; Roma.
- HUANG, S.-S. 1952a, *Ap. J.*, **115**, 529.
- HUANG, S.-S. and STRUVE, O. 1960, « *Stars and Star Systems*, vol. 6, *Astrophysics* », Ed. GRENSTEIN and KUIPER. (We are indebted to Dr. HUANG for sending us a copy of this summary paper prior to its publication.)
- HUANG, S.-S. and STRUVE, O. 1952b, *Ap. J.*, **116**, 410; 1953, *Ap. J.*, **118**, 463; 1955a, *Ap. J.*, **121**, 84; 1955b, *Ap. J.*, **122**, 103.
- JEFFERIES, J. T. and THOMAS, R. N. 1958, *Ap. J.*, 667.
- MCCREA, W. H. 1929, *M. N.*, **89**, 718.
- MENZEL, D. H. 1939, *Pop. Astron.*, **37**, Nos. 1, 2, 3.
- NEVEN, L. and DE JAGER, C. 1954, *BAN*, **12**, No. 451, 103.
- PAGEL, B. E. J. 1959, *Comm. l'Obs. Roy. Belg.*, No. 157, 28; 1959, *Vistas in Astronomy*, vol. III, (London).
- PECKER, J.-C. 1957a, *C. R.*, **245**, 499; 1957b, *C. R.*, **245**, 639; 1959a, *Ann. d'Ap.*, **22**, 499; 1959b, *Liège Colloquium on Astrophysics*, 1959.
- PECKER, J.-C., KANDEL, R., ROUNTREE, J. C. and PRADERIE, F., *Comm. de l'Obs. Roy. Belg.* 1959, Vo. 157; **351**, 36, 43, 47, 53.
- PECKER, J.C. and VOGEL, E. 1960, *Ann. d'Ap.*, **23**, 594.
- RICHARDSON, R. S. and SCHWARSCHILD, M. 1950, *Ap. J.*, **111**, 351.
- ROGERSON, J. B. 1957, *Ap. J.*, **125**, 275.
- ROSSELAND, S. 1929, *M. N.*, **89**, 49.
- SOBOLEV, V. V. 1947, « *Moving Envelopes of Stars* » Russ. ed. Leningrad State Univ.; Eng. trans. by S. Gaposchkin, Harvard Un. Press, 1960.
- STRUVE, O. and ELVEY, C. T. 1934, *Ap. J.*, **79**, 409.
- SUEMOTO, E. 1957, *M. N.*, **117**, 2.
- THOMAS, R. N. 1948, *Ap. J.*, **108**, 130; 1949, *Ap. J.*, **109**, 500; 1952, *Ap. J.*, **115**, 550; 1957, *Ap. J.*, **125**, 260.
- THOMAS, R. N. and ATHAY, R. G. 1961, *Physics of the Solar Chromosphere*, New York, N.Y.
- UNNO, W. 1959, *Ap. J.*, **129**, 338.
- WRUBEL, M. 1949, *Ap. J.*, **109**, 67.

## PART I.

# Questions of General Background and Methodology Relating to Aerodynamic Phenomena in Stellar Atmospheres.

### Discussion.

*Chairman:* M. MINNAERT

— *Editor:*

Following Pecker's presentation of the preceeding paper, there were a number of questions raised by aerodynamicists simply on clarification of the astronomical jargon. Rather than reproduce this somewhat repetitious discussion, its context has been incorporated into the symposium proceedings by simplifying and expanding the relevant sections of the paper. Then, we pass to those aspects of the discussion concerned with the content of the paper in its bearing on the symposium.

— A. UNSÖLD:

Speaking of methodology in the philosophical sense, I am reminded of A. COMTE, who thought that the very principles of scientific inference would make it forever impossible to determine, *e.g.*, the chemical composition of a star.

Looking over some of the examples given of particular modes of astrophysical inference, I would remark that historically, things evolved in a rather different way.

As an example of quasi-empirical, quasi-theoretical inference, the discovery of «chromospheric turbulence» is quoted. Actually, this evolved as follows. I tried to explain (*Ap. J.*, 69, 73 (1929)) the large width of the chromospheric Ca-*H* and *K* lines as Doppler effects due to «turbulence», the word then being used in this connection for the first time, noting however that the motion might also be related to those of prominences. McCrea now noticed that the Pannekoek-Minnaert eclipse measures of a low density, or rather emission-gradient, in the chromosphere could be explained by including the hydrodynamic pressure of the mentioned «turbulent» motions.

As an example of wholly «theoretical» inference, the discovery of the hydrogen convection zone is quoted, whereas the actual happening differs.



The earliest suggested energy transport in stars, by convection, was shown by K. SCHWARZSCHILD in 1905 to be inferior to radiation. But in 1931 I wondered how a static solar atmosphere in radiative equilibrium could exhibit sunspots and other signs of violent motion. The solution came from two sources. First, R. H. FOWLER had noted that ionization of an abundant element could depress the specific-heat ratio, so eventually start convection. Second, H. N. RUSSELL had shown it probable that hydrogen was much more abundant than hitherto supposed. Combining these two ideas immediately showed that the sun must have a convectively unstable layer.

Summarizing these historical notes, it becomes obvious that in all these cases just one, rather trivial, method was followed; namely, to find connections between facts or theoretical ideas which hitherto had appeared unrelated.

Turning to the astrophysical problems, the essential point about which the discussion centers is that in a stellar atmosphere we have two functions. One, the absorption coefficient, depends on frequency, temperature, pressure. The other, the source-function, also may depend on frequency and temperature. It seems to me that this three-term representation of the source-function summarized by PECKER is essentially equivalent to the phenomenological presentation previously used following essentially the procedures of K. SCHWARZSCHILD. That is, the source-function is divided into one part called true scattering, as an extreme case of non-equilibrium; and incoherent scattering, as an intermediate type; and one can add «extinction», a term indicating that the re-emission in a line follows the general behavior of thermodynamic equilibrium, but with a different scale-factor.

How can we find out something about these functions? I think that besides attacking the problem from a more or less formal viewpoint, another procedure may be not bad; viz., simply calculate a great number of different cases and see which depends on which. For example, we ask what is the influence of the kind of radiative exchange on the center-limb variation of the line-wings, examining the question from the scheme of true absorption, scattering, or extinction. Next, we ask what affects the dependence of the effect upon depth, or frequency, for weak lines, or for strong lines. All these points actually have been investigated in much detail.

On the other hand, one investigates the effect of a temperature gradient, of various kinds. Also, what deviations from thermal equilibrium can be well-matched simply by a suitable scale-factor on the Planck function? One can see quite clearly that some observational effects are strongly affected by deviations from thermal-equilibrium, and others not; so he can select a certain body of observations to investigate the effect. Of course, beside such a phenomenological approach, the kinetic approach — favored recently by some workers — remains important. I would emphasize that non-equilibrium calculations are very sensitive to whatever approximations one

makes to the very detailed knowledge required of all the atomic parameters that enter. Kinetic calculations require knowing which process is the fastest, and which the slowest; missing one, everything may be completely wrong in a comparison with observations.

A typical example is the well-known story of the planetary nebulae. If one assumed that the general behavior of a planetary nebula is wholly determined by hydrogen, which is by far the most abundant element, he would obtain electron temperatures about  $10^5$ , corresponding to the color temperature of the central star. Actually, the very few oxygen atoms present depress the temperature to the order  $10^4$ . Such things can happen in ordinary stellar atmospheres, only we don't know yet. We recognize only quite a number of problems — *e.g.*, the Fe spectrum is so horribly complicated it is almost out of the question to deal with it kinetically. But why not deal with hydrogen? But how can we be sure that something is not going to happen, as in the planetary nebulae? I say this, not to deter anyone from making kinetic calculations — that would mean a complete misunderstanding of my remarks — but simply to indicate how terribly complicated they are, and how careful one must be. I would in general favor first a general idea concerning the importance of various kinetic processes and then try to justify or reject the more phenomenological approach.

For the aerodynamicists, I may use a comparison. If you have a complicated problem of turbulent flow, etc., you will probably never think of treating that directly by kinetic methods. But what one does is to justify the phenomenological Navier-Stokes equations, using kinetic methods, then for practical applications uses these phenomenological methods. To me, such a similar procedure in astrophysics doesn't seem bad.

— J.-C. PECKER:

First, I would note that we tried to describe only the logical structure of the various points treated, not looking too deeply into the historical pattern; maybe we were wrong.

Second, on the form of the source-function, a great deal of work can of course be done using the classical approach — using pure absorption, then pure scattering, etc. I would just give an example from the Meudon laboratory, worked out by Mme. PRADERIE (\*), which shows how things can really be understood at once if one just uses the direct  $S_s = B_\nu(T_{\text{ex}})$  approach. I measured several years ago equivalent widths of molecular CH lines, from center to limb on the disk; LABORDE at Meudon has recently made more accurate measures; the results are the shaded area in Fig. 1. How to compute this variation, from theory? Theoretical computations, using the classical

(\*) PECKER, J.-C. and EUGENE-PRADERIE, F., *Ann. Ap.*, **23**, 622 (1960).

theory for pure absorption (equivalent to LTE) gives curve *A*. Pure scattering, gives curve *S*. The classical theory depends greatly upon choice of the model, and we could discuss this at length. But, I have no procedure

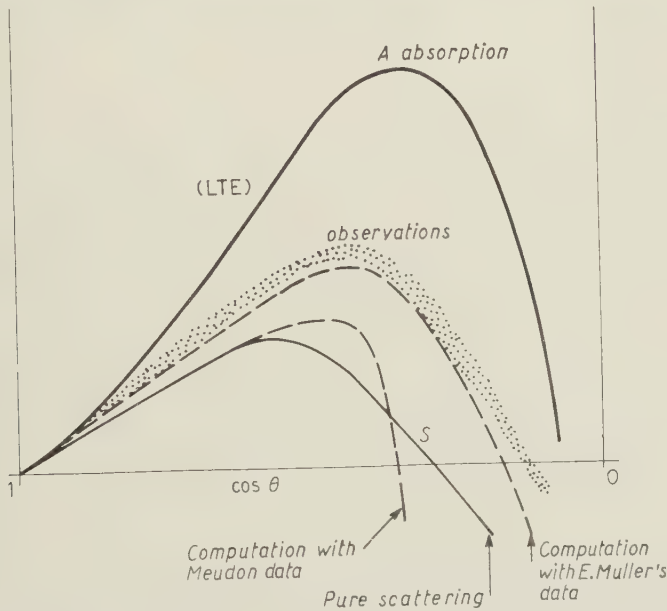


Fig. 1.

whatever that allows a choice between curves *A* and *S*, to distinguish between the effect of a choice here and of uncertainty in model, for such a complicated thing as a molecule. Now, however, turn to the new empirical method which has been set up to get  $T_{ex}$ , on the basis of the methodology described in my talk. It is an iterative method, giving you  $T_{ex}$  as function of optical depth, using the observed central intensities of some CH lines (cf. Fig. 2). From this set of  $T_{ex}$ , and  $B_\nu(T_{ex})$  for the source-function one computes without further hypothesis the center-to-limb variation

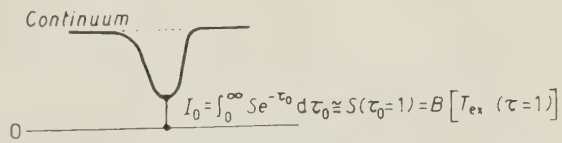


Fig. 2.

of equivalent width. Such computation has been done for two sets of measures; one, by Mme. PRADERIE at Meudon; the other, by E. MUELLER at Michigan. (The former set required a small correction for instrumental profile: the Michigan measure are undoubtedly better.) I give results from both sets of measure, in Fig. 1, to show that the result is quite sensitive to the data.  $T_{ex}$  lies about 300° higher than  $T_e$  at a certain depth from the Meudon data; and about 200



higher from the Michigan data. Possibly the agreement with observations makes me overoptimistic. But I want to emphasize that when you treat the source-function as  $B_\nu(T_{ex})$ , you automatically take into account the interconnection of the source-function with the population of levels, which gives you a way to get  $T_{ex}(\tau)$ , and this result is impossible to achieve with the « classical » method.

— M. MINNAERT:

I would summarize this interchange as follows. According to the situation, it may be advantageous: either to follow the *inductive* viewpoint and derive from the observations the source-function and atomic level populations; or in other cases to follow a deductive method, to start from models described by absorption or scattering and see how the empirical observation agrees; in still other cases the *kinetic* methods may be important, which make use of transition probabilities and collision cross-sections and are the final aim. While we can only be finally satisfied by a kinetic explanation, it is clear that this is for the moment very difficult, and one of the other methods may be of greater advantage.

— A. UNDERHILL:

Most of the remarks made so far deal with the sun. It is possible to observe an isolated point on the sun; for the stars, you always receive the light from the whole disk. Therefore you always have one more integration to invert, and relating the observations to the theory becomes one stage more difficult. Next, considering the considerable observational uncertainties in details of line profiles, I wonder how much meaning can be attached to some of the intricacies of the theory which has been presented. How different are the predicted profiles from a simple theory, say LTE, and those from a more elaborate theory, which is certainly more correct? I agree that we generally simplify the physics because we cannot handle the more correct representation — but you must also remember this point is observational uncertainty.

— M. MINNAERT:

Here, I think lies the advantage of the method favored by Unsöld because then you have calculated several possible models, and have derived the experimental consequences for each of these theories, so you can nicely see where the discrepancies with the observations exceed the errors of measurement.

— A. UNSÖLD:

It is a fortunate circumstance that using for theoretical interpretation of observation the two extreme cases of radiative exchange — true absorption

and then scattering — the differences for most types of stars come out of the same order of magnitude as the usual errors of measurement. This seems to indicate that in stellar work of medium accuracy it is often not worthwhile to worry about fine details of radiative transfer, but just to go ahead.

— R. N. THOMAS:

I will give specific examples to the contrary. Would you admit Lyman-alpha in the sun as a good example or should I go to the stars?

— A. UNSÖLD:

I was only talking of stellar spectra of fairly normal types and in the usual visual range.

— R. N. THOMAS:

Then we should be in agreement that using solar observations, it is easy to distinguish between the LTE and the non-LTE predictions; between the classical method of assuming that a line is formed in some intermediate between pure absorption and pure coherent scattering, and a more precise detailed theory such as PECKER and I summarized. Thus it is not a question of what theoretical approach is *correct* — we can check that on the sun — but whether the detailed theory is too refined for the accuracy of stellar observation.

Turn to the stellar observations of  $\text{Ca}^+ H$  and  $K$ , by O. C. WILSON, whose interpretation is of strong interest to this aerodynamic-astrophysics colloquium. The observations show an absorption line with a self-reversed, emission core ( $M$ -shaped core). Observationally, the separation of the emission peaks increases from hotter to cooler stars along the spectral sequence. A number of authors have interpreted this relation to imply an increase of «turbulence» from hot to cool stars, assuming that the fine details of a transfer problem can be ignored so that the profile of the central «absorption core» simply reflects the profile of the absorption coefficient. If one wants to follow the logic suggested above, he would proceed to ask into a better interpretation of such a profile by saying he has three choices: either interpret on the basis of pure coherent scattering, or on the basis of pure absorption — that is LTE — or try to get a complete non-LTE theory. The questions the theory must answer are: how does the amplitude of the emission peak and the relative amplitude of the emission peak to the emission minimum depend on physical quantities actually in the atmosphere — velocity fields, electron density, etc. I assert that if I use coherent scattering alone, I will not predict the presence of the emission peak. If I use LTE, then I must assume that the temperature is first low, then high, then low going into the atmosphere (since  $I(r) \rightarrow B_r(T_c(\tau_r))$ , Sections 2'3, 3'1 of our summary-paper). On the other hand, going to the non-LTE interpretation, one comes to the conclusions

that the features are explained by a monotonic outward rise in atmospheric temperature, and the relative amplitude of the emission peaks plus the separation of the emission peaks is a strong function of the temperature gradient in the atmosphere in addition to whatever velocity effect may exist. This picture would not come from either of the alternative two procedures, and it is hardly based on theoretical differences comparable to the observational uncertainty of the data.

— A. UNDERHILL:

To obtain emission — you have to have either an extensive body of gas bigger than the star itself, or a temperature that does not, as in a normal star, increase steadily inwards. Now your first two cases, which are very simple, can't possibly give a line like that. Therefore when you have an emission or something peculiar — you know that you have not just a simple case.

— R. N. THOMAS:

I only remark that here we have a good example — involving many stars — where the stellar observations are quite good enough to show the inadequacy of the several simple theories you would have us generally use.

— M. MINNAERT:

It is clear that if you make in the fashion of UNSÖLD the whole series of models, looking not only to LTE or to non-LTE, emission, absorption, diffusion and so on, but also inserting all possible values of micro-turbulence and macro-turbulence, a comparison with observation gives a very full possibility of judging about the model. Only, the question is whether it will not require a great amount of phantasy to combine all possibilities of nature, not forgetting anyone. On the other hand if you are able to interpret inductively the observed profile and to translate that into the source function and the atomic level populations then of course you must take into account the limit of precision of your observations and see whether you have reached the same precision in your theory. If the theories of Fraunhofer lines both methods of approach have been used. They have both their value and I believe that their respective merits have now been put forward sufficiently.

— K. H. BÖHM:

PECKER has strongly emphasized the difficulties which occur if one has to determine the *source function* in a non-LTE situation.

One should also note that the same difficulties occur with regard to the interpretation of the absorption coefficient in the non-LTE case. Consider



for instance the most general case of a subordinate line for which the source function varies with wavelength. Consider a three-level model of an atom, in which levels 2 and 3 are broadened, and absorption from 2 to 3. Level 2 may be reached by absorption from level 1; but if the source function is not constant within the line, this means there is no complete redistribution of the atoms over the level 2. This means the distribution of atoms over level 2 depends on the details of the radiation field in line 1-2. But the distribution in level 2 certainly influences the absorption coefficient in line 2-3. So one cannot interpret in such a case the frequency-dependence of absorption coefficient without knowing what goes on in the other line. Moreover deviations from the Saha's equation alone would lead *e.g.*, to deviations in the absorption coefficient and not in the source function. So it is my impression we need not only a better understanding of the source function but also of the absorption coefficient in the non-LTE case.

— R. N. THOMAS:

I certainly agree that one should study non-LTE effects on opacity as well as on source-function; I do not know how one could study one effect without studying the other. Indeed, our own studies of chromospheric non-LTE effects concerned opacity before they concerned source-function; we were led from the former to the latter. Except at such large optical depths that one does not see them in the line, a non-LTE effect on opacity invariably leads to one on source-function. I would only disagree that in the case you cited, the source-function is necessarily frequency-dependent. I would expect it to be  $\nu$ -independent at least in the line-center, fixed wholly by non-coherence induced by the thermal velocity field; and any other broadening of the energy levels simply introduces more non-coherence in scattered radiation, thus less  $\nu$ -dependence in source-function.

— E. BÖHM-VITENSE:

PECKER mentioned the method of determining velocity fields from the absorption coefficient, as used by DE JAGER and GOLDBERG, and later by UNNO to determine the depth-dependence of the velocity field in the sun (cf. Section 3'1.1) I would emphasize that without knowing something *a priori* about the distribution of line-absorbing atoms, we cannot say to which level the measured velocities correspond. As PECKER pointed out, the method is probably useful for a Milne-Eddington distribution; viz., no depth dependence of ratio of line to continuous opacity and of profile of line-absorption coefficient. However, for a Schuster-Schwarzschild distribution — all line-absorption atoms concentrated in a thin layer — then all velocity averages are performed over this thin layer. However, the layer to which one sees at total optical depth  $\tau = 1$  may lie below this thin line-absorbing layer.

Thus, the method gives  $\bar{x}(\Delta\lambda_i) = \bar{x}(\Delta\lambda_j)$  from one pair of points on the two lines, leading to a  $\bar{V}$  which we assign to the geometric level corresponding to  $\tau=1$ . Another pair of points of equal intensity may correspond to a  $\tau=1$ , thus to another geometric level, to which we assign the  $\bar{V}_1$  determined from this pair of points. But, both  $\bar{V}$  and  $\bar{V}_1$  represent averages over the same, thin, line-producing layer; and if we do find a change in velocity, it would mean that there is a *very* steep gradient in velocity in the high atmospheric layers, in the case of our example. This situation is true mainly for neutral atoms of low excitation. So one must be very careful in using this method.

— M. MINNAERT:

Since in your example, the optical thickness of the line-forming layer must be less than 1, would you agree the method becomes useful for thicker layers?

— E. BÖHM-VITENSE:

No, because you still measure the mean of the velocities down to a certain point, and you still don't know the depth to which the mean refers. Also, in many cases you cannot very well compare the two lines because you don't take the mean over the same regions.

— J. WADDELL:

PECKER considered two points I should like to comment on. The first is the identity of the source function in lines of the same multiplet; the second is the frequency independence of the source function. Both of these points are subject to observational checks. Consider the case of the Na *D* lines. The emergent intensity on the disk of the sun for Na *D*<sub>1</sub> or Na *D*<sub>2</sub> is given by — using *j* to denote which line, and assuming a common source-function —

$$I_j(\mu) = \int_0^\infty S(x) \exp[-\tau_j/\mu] d\tau_j/\mu,$$

At any geometrical depth, *x*, the optical depth  $\tau_2(x) = 2\tau_1(x)$  because the *f*-values of the two lines are in the ratio of 2 to 1. In this case

$$I_2(\mu) = I_1(\mu/2).$$

This equation should break down in the wings of the line where the source function of the continuum becomes important and where the damping enters.

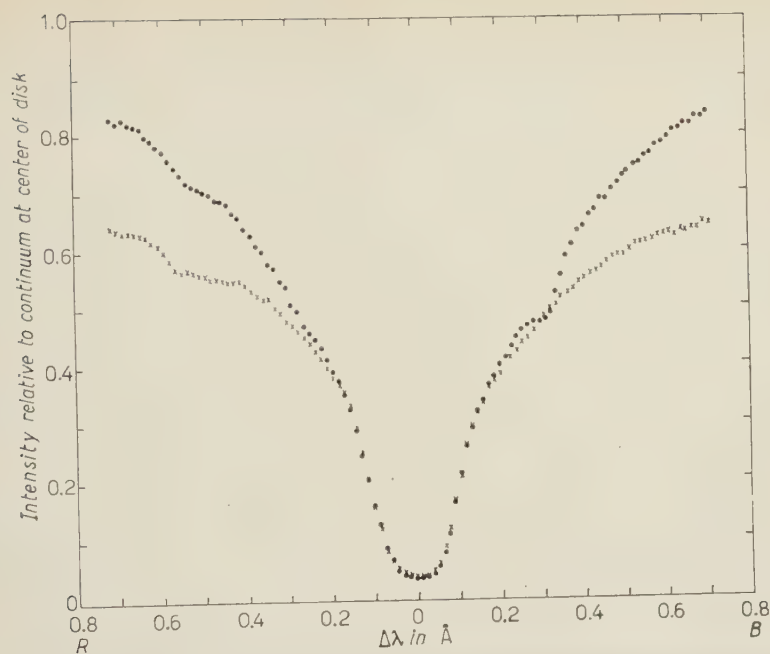


Fig. 3. - Comparison of  $I_2(\mu, 0)$  with  $I_1(\mu/2, 0)$  in core of lines (not corrected for telluric absorption).  $\times$  Na  $D_1$ ,  $\mu = 0.50$ .  $\bullet$  Na  $D_2$ ,  $\mu = 1.00$ .

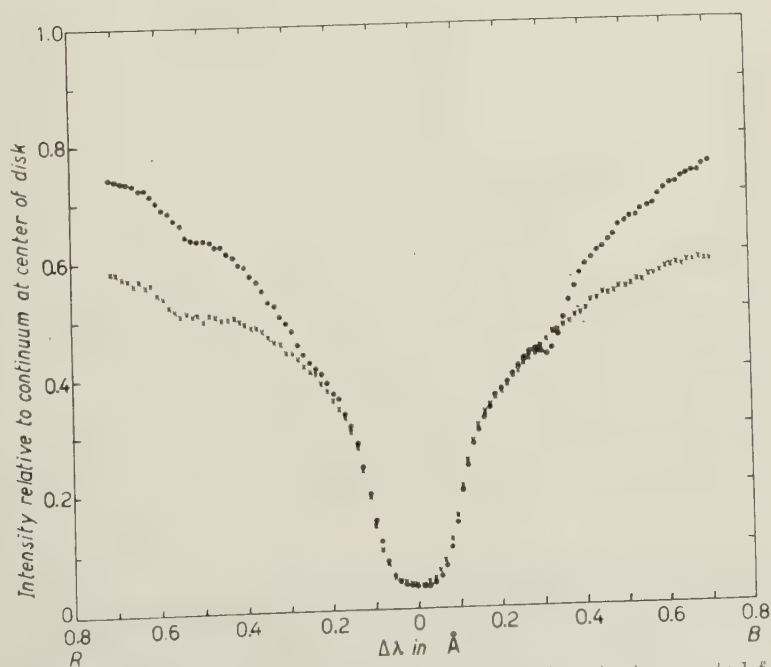


Fig. 4. - Comparison of  $I_2(\mu, 0)$  with  $I_1(\mu/2, 0)$  in core of lines (not corrected for telluric absorption).  $\times$  Na  $D_1$ ,  $\mu = 0.40$ .  $\bullet$  Na  $D_2$ ,  $\mu = 0.80$ .



At the Sacramento Peak Observatory I have made center-to-limb observation of the Na  $D$  lines where the  $\mu$ 's were chosen in the ratio of 2 to 1. Figs. 3, 4 and 5 demonstrate the comparison. It would appear that the

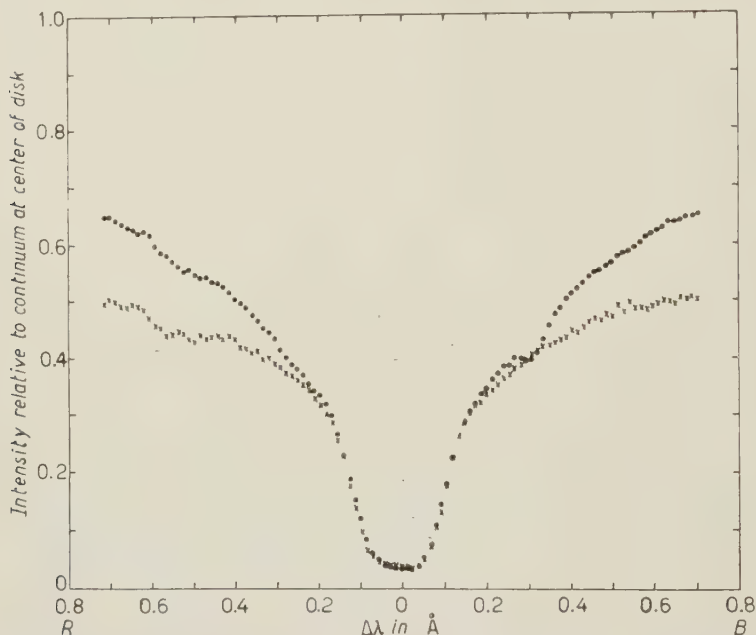


Fig. 5. — Comparison of  $I_2(\mu, 0)$  with  $I_1(\mu/2, 0)$  in core of lines (not corrected for telluric absorption).  $\times$  Na  $D_1$ ,  $\mu = 0.30$ .  $\bullet$  Na  $D_2$ ,  $\mu = 0.60$ .

assumption that the source function of the Na  $D$  lines is identical, is a reasonable one. Next we use the intercomparison method at a particular  $\mu$  to derive the Doppler width. Fig. 4 (*Ann. d'Ast.* **23**, 921, (1960)) demonstrates the values of  $\Delta\lambda_D$  derived for various intensities at the given disk positions  $\mu$ . Goldberg's results for the Ca<sup>+</sup>  $H$  and  $K$  lines are compared. In both the  $H$ ,  $K$  lines and the Na  $D$  lines at  $\mu = 1.00$ , we find that the Doppler width increases with depth into the sun and yields temperatures in excess of  $100\,000^\circ$ . GOLDBERG suggests that the intercomparison method is invalid if the source function is frequency-dependent; I feel this must be the case because we have excluded for the Na  $D$  lines that the fault might lie in different source functions for the two lines.

However, it would appear that the source function is completely non-coherent (*i.e.* frequency-independent) over the region where  $\Delta\lambda_D$  is flat, namely for intensities up to 15 per cent of continuum. The Doppler width obtained in this region is on the order of  $.04\text{ \AA}$ , yielding a temperature on the order of  $5\,400^\circ$ . On the other hand, a temperature of  $4\,000^\circ$  would

permit a random «turbulent velocities» on the order of 1.8 km/s. This tentative conclusion is in agreement with UNNO. His results based on McMath-Hulbert Observatory tracings indicate also a decrease of turbulence with height.

— K. H. BÖHM:

I find the 3-term form of writing the source function very convenient if one considers resonance lines or very special types of subordinate lines. But for the general case of subordinate line, I cannot understand that it is useful to make such a distinction between lines of photoelectric type and collision type. Consider a many-level atom. The emission coefficient in line  $(n+1) \rightarrow n$  is coupled to all other possible transitions within the atom, and therefore to the radiation field in all lines and the continua. But this coupling is usually not expressed explicitly (though it is certainly implicit) and I do not see how such a line can be characterized as either photoelectric or collision type. It has often been asked whether radiative equilibrium with coupling between many different levels would not under certain conditions lead to level populations close to the case of LTE even if the radiation field in the different lines is not given by the Planck function.

To answer for instance this question one might perhaps prefer a different formulation of the source function like *e.g.* that of HENVEY, which within a certain transition frame of approximation shows the coupling between the different transitions explicitly.

I should like to ask THOMAS whether he thinks that his source-function describes any situation or whether it is his own opinion that he really wants to have its application limited mainly to resonance or to certain types of subordinate lines.

— R. N. THOMAS:

I share your concern — and believe this is one of the chief problems facing us in discussing the source-function. Certainly everything we have done up to now is only for resonance lines — really only for a 2-level atom plus a continuum. Now there are two alternate ways of trying to extend the methodology that JEFFERIES and I have introduced for the resonance lines. On the one hand, one could do explicitly just what you mentioned, try to take all the transfer problems in all the lines into account. JEFFERIES has set up a chain-process-type attack in an attempt to investigate this problem (*Ap. J.*, 1960) in certain simple case. It essentially comes down to a set of simultaneous radiative transfer equations, which are very messy. From my own standpoint, I prefer to try to reduce the problem to an «equivalent 2-level atom» — write down the equations of statistical equilibrium for the 2 levels, taking into account all the transitions then try a perturbation

treatment. The source-function is a ratio of 2 absolute rates; emission in the line : the absorption in the line. Retain these, and transitions to which the atmosphere is transparent, as absolute rates; introduce other radiative processes, requiring solutions of a transfer equation, as net rates, which you evaluate as perturbations, by iteration on the two levels to which they correspond. Therefore we have reduced these 2 exact equations of statistical equilibrium to an equivalent 2-level atom, using exactly the physical idea of the three terms we had before: one term is the radiation in the line itself: a second is the collision in the line itself — those are the only 2 direct processes; any other process, ionization and re-capture, or excitation to a higher level and cascade, is an indirect process, which you treat in exactly the same way as you did the terms to the continuum in the 2-level (plus continuum) approximation.

— K. H. BÖHM:

Let us assume you use the iterative procedure for calculating a particular subordinate line, say the Paschen  $\alpha$  line, corresponding to a transition from  $n=4$  to  $n=3$ . Now for an atom in the level  $n=4$  usually the probability for making a transition to, say  $n=2$  or  $n=5$  is of the same order of magnitude as the probability for making a transition to  $n=3$ . Now you start with a 2-level atom consisting of  $n=3$  and  $n=4$  and use an iterative procedure, though the probabilities of transitions to other levels are of the same order of magnitude. I don't see how this procedure converges.

— R. N. THOMAS:

The idea is that you express the 3-4 transition as an absolute rate, and the 2-4 transition as a net rate; thus the *relative* size of the latter becomes very small. If I can get a rough approximation to its value, there is a *hope* that error will not perturb the solution too much. Work along these lines has just started. Some of it is described in Chap. 9 of the Chromosphere monograph (THOMAS and ATHAY (1961): cf. bibliography in PECKER-THOMAS paper).

— G. ELSTE:

First, a remark on the question of a more realistic model of distribution of absorbing material, raised by Mrs. BÖHM-VITENSE. The cores and flanks of medium strong lines are formed in a layer about 150 km thick, compared with a thickness of about 300 km for the whole photosphere. Plot the contribution of each depth to the  $I_\nu$ -integral, *vs*  $\log \tau_c$ . You find bell-shaped curves, with half-width about  $\Delta \log \tau_c \sim 1.4$  (Fe,  $\chi \sim 4$  eV). For a line twice as strong, the position of the contribution curve shifts upward about  $\Delta \log \tau_c = -0.4$ . So the layers over which one has averaged in the two cases overlap considerably. The  $\log \tau_c$ , to which the  $\Delta \lambda_D$  found by Unno's proce-



ture refers, may be the center of gravity of the contribution curve. This is not necessarily the depth at which the value of source-function equals the emergent intensity, as used by UNNO.

Second, I have a result bearing on a choice between depth-variation of velocity and  $\nu$ -variation of source-function. Restrict attention to such weak lines that the only broadening mechanism for the absorption coefficient is Doppler; use  $T_e(\tau_0)$  obtained empirically from the continuum; and assume  $S_\nu = B_{\nu_0}(T_e)$  to compute line profiles, without introducing any turbulent velocity. These computed profiles are only some  $\frac{2}{3}$  as wide as those observed. One choice for an explanation lies between introducing non-thermal motion and introducing a  $\nu$ -independence for  $S_\nu$ .

I think, a priori one can say that the total source function in the line  $(S_s + r_\nu S_c)/(1 + r_\nu)$  can differ from that in the continuum, and therefore can have a  $\nu$ -dependence, only where the total absorption coefficient departs non-negligibly from the absorption coefficient in the continuum. If one has a thermal motion according to the kinetic temperature  $T_e$  in the photosphere, the line absorption coefficient — which has to be added to the continuous absorption coefficient — has a width which is only  $\frac{2}{3}$  of the width of the observed line. How can the source function differ from that in the continuum over a range larger than the one where the absorption coefficient is different? This is only possible if there exists an additional widening such as non-thermal, so-called «turbulent» motions provide. So as a first approximation, a non-thermal velocity field with two unequal components, horizontal and vertical, each depth-independent has been introduced (ALLEN, WADDELL). These two quantities are fixed by two observed quantities, central dip and half-width. We are able to fit the center-to-limb variation of the equivalent width and half width and to reconcile computed and observed profiles at the limb, but there still exists a discrepancy in the profile at the center of the disk as shown in Fig. 6. The question is do we revolve this discrepancy by introducing a depth-dependence of the vertical motions, or do we try a  $\nu$ -dependent  $S_\nu$ ? I believe we can exclude the latter unless, in addition to the  $\nu$ -dependence, an anisotropy of the source function is introduced, because the discrepancy only appears at the center of the disk but vanishes at the limb.

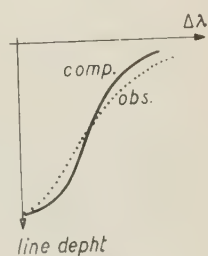


Fig. 6.

— *Ed. note:*

Write

$$I_\lambda = \int (S_\lambda \exp [-\tau_\lambda/\mu] \tau_\lambda/\mu) d \ln \tau_\lambda.$$

Quantity in brackets is the contribution function, on  $\log \tau_\lambda$  scale, for  $I_\lambda$ .

— J. WADDELL:

PECKER spoke of the difficulty in distinguishing between depth dependence and angular dependence in turbulence. Let me show why I believe it is possible to distinguish between the two effects. Consider the contribution function of the central dip of two lines, *A* and *B*, at two positions on the disk ( $\mu = 1.00$  and  $\mu = 0.35$ ).

Line *A* at disk position  $\mu = 0.35$  is formed at the same optical depth as line *B* at disk position  $\mu = 1.00$ . Nevertheless both lines *A* and *B* are represented with a constant radial component of turbulence of 1.8 km/s and a constant tangential component of 3.0 km/s. If the increase of Doppler width to the limb were interpreted as an increase of turbulence with height above the photosphere for line *A*, it would not be possible to compute the correct half width of line *B* at the center of the disk.

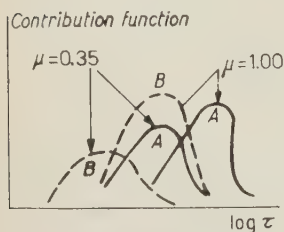


Fig. 7.

Since this somewhat idealized case is similar to the results I obtained at the McMath-Hulbert Observatory, for 11 lines, I feel that one is able to distinguish between the angle and depth dependence. Further, when one looks at the variation of the half widths with  $\mu$ , he finds a strong increase near  $\mu \sim 1$ , leveling off at smaller  $\mu$ , for many wide lines. This is an immediate clue that we are concerned with a  $\mu$  effect and not an optical depth effect; for optical depth effects show themselves most strongly quite near the limb. It is possible to represent the observations with a depth dependence of the form

$$\nu^2 = A^2 - (A^2 - B^2)\tau^2,$$

but unlike the  $\mu$  dependence, the values of *A* and *B* depend on the line chosen. However when I used non-isotropy, I could explain eleven lines with the same 2 constants: 1.8 km and 3 km (cf. WADDELL: *Ap. J.* **127**, 284 (1958)).

— J.-C. PECKER:

In presenting the summary-introduction, I mentioned the possibility of such effects as discussed by WADDELL originating in non-LTE effects. At Meudon, we have been looking at Unno's graph of his photospheric results, giving rms velocity variation with depth, which show that the points coming from the lines of Ti and those from Cr fall systematically on opposite sides of his mean curve. This result is easily interpreted, if we assume UNNO made (implicitly) a mistake, computing the optical depth of each of these lines in the classical way. Now if one introduces non-LTE considerations

from a purely empirical standpoint, he finds that this dispersion of points is entirely due to neglected non-LTE effects (J.-C. PECKER and L. VOGEL: *Ann. d'Ap.*, **23**, 594 (1960)).

— J. WADDELL:

Note that previously the increase in Doppler width with decreasing  $\mu$  had been explained as a depth-dependence — turbulence *increasing* with height in the photosphere. Unno's results go just in the opposite direction. I do not think we should argue whether he is right or wrong. I would only note that his results are a smaller magnitude motion, compared to those from line-broadening to the limb. Unno's results do not explain my observations, nor are they an alternate picture. Also, they are based only on observations made at the center of the disk. Our interpretations are compatible.

— M. MINNAERT:

There is one point which was mentioned in the speech of PECKER where the aid of the aerodynamicists will be especially useful for us. That is the question whether supersonic turbulence is possible; we have an aerodynamicist prepared to give his opinion on this point.

— F. H. CLAUSER:

When asked if I would talk about whether supersonic turbulence were possible, I said that I was not prepared to answer the question. MINNAERT said it's always the same with aerodynamicists, that any question we ask them has not been worked out, so give your beliefs.

In order to make any meaningful statement, I feel that I will have to go back a bit with our concepts to lay a little foundation. Suppose that we were to be confronted with a velocity field, of a single component species. Later on I'll come back and try to give an indication of what happens when many species are present. Such a velocity field for this single component is specified by the three components of velocity:  $U$ ,  $V$  and  $W$ , — each as functions of  $x$ ,  $y$ ,  $z$  and  $t$ . But for simplicity of illustration in the example that I shall quote to you, I shall assume that there is a single velocity component,  $U$  and that it will be a function of a single variable  $x$ .

I try to avoid those questions that mean the difference between a vector variable in a vector space with a time variable parameter, and those which you can talk about, a scalar variable in a scalar space. There are many problems which revolve around the difference between vector and scalar variables, but I think I can avoid these without any controversy. Now — so far as the aerodynamicist is concerned, there is an important fact of life that emerges and covers a good deal of our thinking and background — that



is, those particular common fluids that are given to us to work with, such as air and water, have such a low viscosity that if you were to compute the Reynolds' number from any realistic combination of parameters, you invariably get a very large number. (The Reynolds' number is defined as a velocity times a length divided by the kinematic viscosity — viz., the ordinary viscosity divided by the density of the fluid.) In fact to get Reynolds' numbers of the order of 1, you either have to go to such microscopic sizes for reasonable velocities that you can't use ordinary instrumentation any more, or you have to go to such low velocities with ordinary dimensions that you haven't any instruments capable of measuring them any more. Basically this means that in those characteristic features that we see in turbulent flow, — eddy sizes, etc. if we take any characteristic velocity and any characteristic length, and divide by kinematic viscosity we invariably get large numbers. Now this is the background on which I base the next part of my remarks. You all are familiar with the difficulties of analysing non-stationary random functions. If you simply start with truly non-stationary random functions, it's hard to know just where to begin, and invariably you make assumptions that the random function

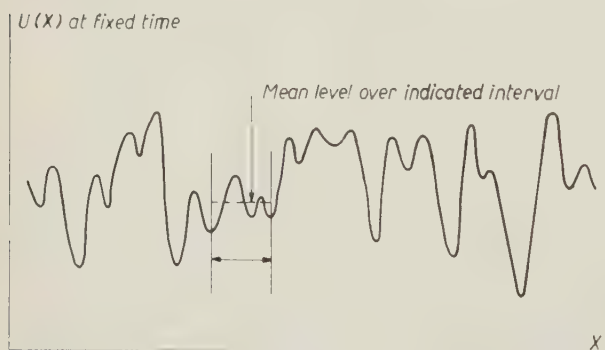


Fig. 8.

must be stationary or some such postulate. Now the way that many of us start is the following. Supposing that we were in fact given this simple plot: one velocity component against a single variable  $x$ . If we take a cut at a given instant of time through the fluid and measure the result, we get some such random function as this.

Now supposing, to be definite, we were to select, to begin with, a characteristic size — which I will vary in a moment — but pick out a given size and say that we shall analyse a sample of this length. We see that its velocity has a mean level, and around that mean it has fluctuations. If we look at the dynamics of such a system, we find that if we consider those particular parts of a fluctuation that are small compared to this length, then from a physical point of view I have an eddying motion taking place. The fact that I've picked the mean here, as the start, implies that I no longer ask about an absolute magnitude of velocity but simply have a glob of matter here in space that is undergoing an eddying motion, and I walk along with it, so that it looks to me as though it's standing still on the average. And within that chunk of matter, there is a complete hierarchy of eddies. Since I've restricted

my consideration to something this big or smaller, I can't talk about bigger eddies. They do not exist for me — I've simply cast them out by simply agreeing to go along locally with this glob. You are all familiar with the classical concept of viscosity and what it means. If I have a streaming motion in which different layers are moving at different velocities, and a sub-motion takes place — it will carry with it momentum from one layer to another. Now, in this problem that I consider, I find that there are these small-scale motions taking place, doing exactly this interchange of momentum, exerting an effect of viscosity. Let me tie this in now with my fact-of-life of a moment ago, that the ordinary, thermal-motion, viscosity is quite small. Physically what is happening is this: I have a glob, which has a characteristic motion within it that is distinguishable if I look at sizes that big, but there is a random smaller motion, still large in scale compared to the thermal motion, but much more important in transferring momentum within the glob than is thermal motion. This smaller sub-motion is acting as a viscosity to the glob itself. In this way energy is transferred from the large-scale motion down the size-scale to thermal motion. We express this quantitatively in terms of what we call a power spectrum. It is essentially the square of the amplitude of the ordinary part of the spectrum. Plot here the wave number against the energy per unit wave number size, and we find that for a characteristic turbulent motion we get a curve that looks something like this.

I've been talking about what goes on in a central portion of the spectrum — the part indicated by an arrow. It is an empirical fact backed up by a number of theories that  $F$  varies as  $n^{-3}$  in this region. This result emerges purely dimensionally if you make the assumption that all of the energy transfer is taking place by the smaller eddies acting as viscosity for the larger hierarchy.

But now consider the two extremities of the curve. First if I consider large  $n$ , small eddy size, I find  $F$  decreasing continually. If I multiply the amplitude — which is a kind of velocity — by  $n^{-1}$ , which is a kind of length, I can form a kind of local Reynolds' number, which gets smaller and smaller. There comes a place where this is of the order of the number computed from thermal viscosity.

It is at that point that the energy is no longer transferred from the bigger eddies to the smaller eddies, but now begins to be transferred by viscous action to heat. So we have the concept: energy in the big eddies gets transferred to even smaller eddies down the chain, and eventually is converted into thermal energy.

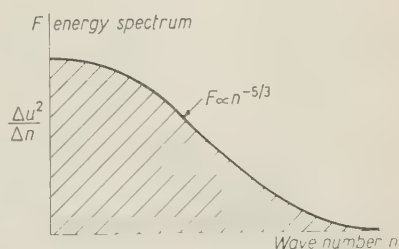


Fig. 9. — Power spectrum.

But at the other end, if I progressively increase my length scale — invariably I come to the scale where there is something feeding the energy in. In laboratory experiments it is invariably a solid wall, an obstacle, a wing, a propeller blade, or it can be convective eddy sizes, Benard cells. It is at this point that the statistics seem no longer to be appropriate; you get a field that you do not analyse statistically any more. You begin to search for the non-linear driving mechanisms that feed the energy from a moving, solid wall, or a convective cell, etc. That dimension, we are missing. Our representation breaks away from this curve when you reach sizes that are particular to the boundary layer, the wave size, the cell size, etc.

So far it looks as though I am not talking at all about supersonic turbulence, but I had to get this background laid for it. Now, we have this velocity field, which so far I have treated as a continuum. Supposing that we were to use even smaller instruments in making our measurements. If we were to do so we would find that the curve does not look like this. To be specific, supposing that I were to introduce a little cork ball in the fluid, and actually trace out its motion in order to determine the velocity field, then introduce progressively smaller and smaller ones. Now, to a certain range, I will map out this velocity field, but as the ball gets small enough there comes a time

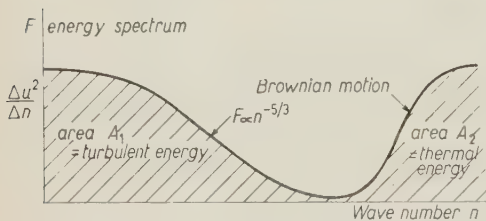


Fig. 10. — Power spectrum.

where the Brownian movement begins to be perceptible. Then I get a change in the curve, a tail is added for large  $n$  — above the tail, I have what I call turbulence; below it, I have the thermal motion.

Now, this is essentially  $\Delta u^2/\Delta n$  plotted against  $n$ . So the area under this « turbulent » portion of

the curve,  $A_1$ , gives me the turbulent energy,  $u^2$ . The area under this « thermal » curve,  $A_2$ , gives me the velocity of sound squared,  $a^2$ .

Now the question about supersonic turbulence in our view arises in the following way; the turbulence would be supersonic if this area  $A_1$  became larger than this area  $A_2$ . Is this possible? And what new concept would happen if it were to become so? Now in the first place, all of our laboratory experiments have been in cases in which this area  $A_2$  is considerably smaller than  $A_1$ ; the ordinary cases of subsonic turbulence.

But let us look and see what happens now if we were to approach the case of supersonic turbulence. I would do this not by making  $A_1$  bigger — rather hold it constant, simply decrease the temperature, and see what happens. When looked at this way, there is a unity that did not exist before. The energy of one eddy was fed into a smaller eddy, and a chain arose. But there came



a break in the chain; this part of the curve at the break was not drawn: I simply said I had to invoke a new mechanism, viscous dissipation, which fed the energy into the thermal part.

Now I do not have to do that. I can talk about this whole process as part of a single curve, and simply say that the energy is being continuously transferred down the curve and into that portion of the curve which is the thermal energy. So I can apply the whole process throughout the entire curve. If we go toward supersonic turbulence, not by making the velocity larger but by making the velocity of sound smaller, the amplitude in the thermal region will come down. There is a coupling between these two regions, so the amount of viscous dissipation at the onset of the thermal region is reduced. So in the process of following another curve in the thermal region, the position of the curve in the turbulent region is raised (cf. Figure). To reduce the viscosity, all you do is to have the eddies extended to smaller scale. So the net flow of energy down the cascade of eddies is still the same. As we continue to decrease the viscosity, the «minimal» position of the curve simply moves out even farther.

Eventually, the smallest eddies become comparable to the mean free path and there is really no clear distinction between eddies and thermal motion anymore.

Originally, almost all work was done on incompressible turbulence. The advent of high speed flight forced us to consider the effects of compressibility on turbulence, and as usual, the effect of compressibility wasn't just to shift thing a little — it meant going back to some very fundamental ideas. You go through the formal process of setting up the Navier-Stokes equations, including the conductive and convective heat transfer equations, the equation of continuity, etc. — they are horribly non-linear, almost hopeless to try and solve. But for small perturbation of the flow, you make the following assumption — you assume that essentially you have a uniform background field on which you superimpose fluctuations of temperature, pressure, density, velocity, etc. This gives you a set of partial differential equations of high order, which you can linearize by saying that the fluctuations are small compared to certain basic quantities — *e.g.* the velocity of sound, the fluctuation must not be large compared to the basic temperature, etc. And in this process you find the high order partial differential equations can be split so that the total field of variation can be represented as a set of solutions of several different partial differential equations.

We interpret that as saying that the full compressible Navier-Stokes equations have solutions that represent different modes of behavior. Now if you examine these modes of behavior, you find that one of the equations is essentially the equation of sound modified by certain small terms in which you have simultaneously occurring density fluctuations, temperature fluctua-

tions, velocity fluctuations, etc. In addition there is a second set that essentially corresponds to shearing motion. And these equations have all the characteristics that we have associated previously with incompressible turbulence. There is a third set of equations that essentially correspond to heat transfer, but this is a convective type of heat transfer, and it is not ordinary heat transfer in which there is no velocity present. It turns out there still are velocity- temperature- pressure-, etc., fluctuations, but in different proportions — the velocity is much smaller now. And it turns out that as long as the fluctuations are small, any random field can be categorized in these three parts — so much eddy-turbulence, so much random-sound, — and so much hot spots which are diffusing. But as one increases the level of the fluctuations, non-linear terms begin to appear, and cause what had been neatly categorized as eddy-turbulence to begin to feed over into the random sound, and also over into the conductive, convective transfer.

Now there comes a point where you can no longer say that this particular thing is eddy-turbulence, and this particular thing is random sound, and that particular thing is a set of hot spots randomly distributed throughout the field. Now we believe that your physical intuition about the existence of turbulence and about the existence of sound waves and about the existence of hot spots and so on is not an arbitrary one; that there really is something in nature, that this mathematical splitting goes into your intuition, and that you find that there is this splitting. But, we think your intuition begins to fail you under strong conditions, where the amplitude becomes so large that you can no longer split those things and say that is sound waves, this is turbulence, and so on.

I should like to say that aerodynamicists have done some additional work on the effect of magnetic fields. We find that as soon as magnetic fields are present that will influence the flow field, the order of the equations increases and they become even more complex. A typical new phenomenon that enters is the waves first discussed by ALFVÉN. These appear as an additional type of mode for the fluid.

So, when we ask if supersonic turbulence is possible, the first question that arises is whether, with such large amplitudes, as would be required by supersonic turbulence, the various modes of behaviour can be separated out, or do they necessarily become strongly intertwined. Also, the question arises as to whether, in supersonic turbulence, the turbulent motion can be separated from the thermal motions, or do they blend together.

In astrophysical examples, it would appear from what has been said before, that an additional complication is present. This is the possible streaming of various constituents or species through each other. In the case of electric currents, we have a single example of such a situation, because the current represents a streaming of the electron through the ion and the

neutral particles. In astronomy it appears possible for the various atoms to stream through each other. For such problems, relatively little work has been done. It is quite possible that if such cross-streaming takes place on a large scale, gross instabilities can occur, and turbulence would appear, driven by other new interaction mechanisms. This will bring in wholly new concepts that are not contained in our present experience.

— M. MINNAERT:

On the case of different kind of atoms, we astronomers would have given as the criterion — different kinds of atoms will have different velocity of thermal motion, inversely as  $(\text{mass})^{\frac{1}{2}}$ , but the same velocity of turbulence.

— G. ELSTE:

One has to be very careful with such a simple criterion. If he deduces different turbulent velocities, or different thermal velocities from different widths of different lines, it is not clear that the results refer to the same atmospheric region. Especially, this is true in extended atmospheres, or prominences, where regions of completely different physical conditions lie along the same line of sight.

— J. C. PECKER:

And, one must be careful in saying non-thermal motions show no correlations with properties of atoms — *e.g.* gyromagnetic motion when magnetic fields exist.

— M. MINNAERT:

Let us not complicate things — neither by magnetic fields nor by speaking of different regions. If we have a small mass of gas, astronomers try to separate thermal from turbulent motions by looking at the difference between lines of different atoms. Must we assume this method is oversimplified?

— E. SCHATZMAN:

I want to comment on Clauser's nice description of so-called supersonic turbulence, in connection with several astrophysical facts or theories. Consider first the problem of the heating of the solar chromosphere by sound waves. It is usually accepted that we have in the convective zone some eddies, that some compression waves are produced by these eddies, and that these compression waves get out and refract in the upper part of the solar atmosphere. After a few wave-lengths these compression waves are transformed into minor shock waves of small amplitude — and we have in this region a random noise of small shock waves. When we use fairly reasonable estimates of the amount



of energy dissipated, we do not find at the shock front a velocity jump larger than 2 km/s. With an amplitude of 2 km/s and a distance of 10 s between 2 successive fronts, we have quite enough energy for explaining the heating of the solar chromosphere. There are minor differences concerning the theory but general agreement.

We are not yet in the region Clauser described as supersonic turbulence: we have compression waves of a quite well understood nature, and if there is any turbulence, it is produced as a secondary effect by these random shock waves, and is a small effect.

Second, turning to the stellar case Struve has observed cases, in which (*Ap. J.* **104**, 138 (1946)) velocities inferred from line-profiles are much larger than one would expect on the basis of results from the curve-of-growth method. It has been interpreted by saying that we have very big masses in motion, and we integrate over the whole disk, like integrating over a series of small atmospheres moving at random at the surface of the star. Then, this profile is actually supposed to be a profile due to macroscopic motion. But what is remarkable is that from the excitation of the lines we have a temperature of the order of  $7000^{\circ}$ , which corresponds to a sound velocity in hydrogen of about 8 km/s, while the profile corresponds to about 25 km/s. This is undoubtedly macroscopic motion with velocities larger than sound velocity. It was a great temptation to extrapolate the above-mentioned theory of acoustic heating of the chromosphere to the case of a giant star with a large convective zone, purely phenomenologically. We suppose a large production of sound waves in the convective zone, and we suppose that the amplitude reached by the shock waves in the region where they are seen in the spectrum is large. But then it is undoubtedly the case Clauser has mentioned, when we cannot distinguish between random shock waves and so-called supersonic turbulence. I would only note that using this phenomenological theory, basing calculation on the assumption of a constant ratio of the mechanical flux to the light flux, and using an elementary theory of the line formation we could explain the width of lines in the case of four stars, the material I had in hand 10 years ago. It would be worth trying again with the material collected by Miss UNDERHILL. Naturally astrophysicists would welcome any kind of improvement of the theory, so that we could abandon this phenomenological theory for a reasonable and sound theory of the random motions in such a case.

— E. N. PARKER:

I would like first to emphasize a point that has been made many times before, when an astronomer uses the word «turbulence», he is not necessarily thinking of the same phenomena as the aerodynamicist. He means any motion that is not microscopic in the kinetic theory sense — but on the

other hand which is of smaller scale than his own visual resolution. It might very well be ordered motion — rising and falling columns, such as Benard cells. With that point in mind, I want to discuss a single case of supersonic motion — let its scale be smaller than our ability to resolve visually so that I may legitimately call it «astronomical turbulence» — I think we can come to a specific conclusion for this special case. Suppose that I have a boundary with a semi-infinite medium on one side, and suitable machinery on the other side of the boundary to generate waves. These waves may be compressional sound waves — they may be transverse hydro-magnetic waves — they may be longitudinal hydro-magnetic waves. The principle in every case will be the same. I generate waves at the boundary and the waves propagate outward into the medium. Let the initial amplitude of the waves be 100 km/s. I use these numbers because they are not inappropriate to the solar corona. Let the temperature of the medium be 8000 °K, so that the thermal velocity is  $\sim 10$  km/s. Now regardless of whether the wave is transverse or longitudinal, if it has such an enormous amplitude compared to the thermal velocity of the gas, it will steepen its front. The steepening will go on until halted by some sort of dissipation mechanism. If the wave were a sound wave, it would quickly become a shock wave. With the Mach number at 10, the temperature behind the shock wave will be much larger than ahead. The thermal velocity would be less, but of the same order of magnitude as, the 100 km/s fluid velocity in the wave. So as the wave sweeps off to the right the atmosphere returns to something like hydrostatic equilibrium. The original pressure is approximately restored and the temperature is some sizeable fraction of a million degrees Kelvin.

If the disturbance were isentropic — the original temperature would be restored; the irreversible character of the disturbance at such a large shock strength lets the medium retain a temperature near the peak value — so long as we neglect electromagnetic radiation.

Now if the gas should radiate fairly rapidly, relative to the interval between pulses, then the temperature which may have jumped a million degrees behind the shock will rapidly settle back to its 8000° equilibrium value. Thus when the next pulse is generated with 100 km/s velocities, it again will be supersonic, Mach ten, and repeat the cycle. Thus in this assumed case, an observer would see a temperature which is most of the time, over most of the area, 8000°. He would find superimposed velocities of the order of 100 km/s. The observer would — in the sense that I said astrophysicists use the term — say that he had supersonic turbulence.

Now on the other hand, suppose that the pulses come sufficiently quickly that the million degrees does not have time to cool down by radiation between successive pulses. Then the next 100 km/s pulse is not very supersonic; it will dissipate energy — but not nearly as much. The next pulse will dissipate

even less, and it will travel, of course, very much farther through the medium before it dies out. The medium remains near  $10^6$  °K. The point that I want to make here is that an observer looking into the gas would now see only sonic motions. He would see 100 km/s material velocities in a million degree gas, which is roughly Mach one. He would not have supersonic turbulence, as opposed to what he would see in the more rapidly cooling gas.

In the case of the solar corona, the temperature relaxation time is very long, somewhere on the order of a fraction of a day. And if one thinks of shock waves or hydro-magnetic waves coming up into corona every few minutes then the cooling between shocks is slight. One has, then, a situation where he would have subsonic turbulence, by virtue of the fact that the temperature will rise to that point where the waves no longer form steep fronts and irreversibly dump energy into the medium. This is the effect which at least some of us think is very important in the heating of such things as the solar corona, what may control the temperature of the corona, the temperature rises to that point where the waves no longer are supersonic. One must worry about this particular condition in any star or situation where he sees what looks like supersonic «turbulence». Can you in fact keep the temperature down enough so that the «turbulence» is supersonic, or will the «turbulence» simply raise the temperature of the medium so quickly that you could not maintain it at a supersonic velocity?

— *Ed. note:*

(For a more detailed discussion of the «piston» problem used as example here, cf. the summary by WHITNEY in Part III-A. For a summary of the viewpoint that «supersonic astronomical turbulence» represents a blend of higher atmospheric temperatures and non-random macroscopic motion, cf. Chapter 1, *Physics of the Solar Chromosphere*, THOMAS and ATHAY (1961)).

## PART II.

# General Summary of Results on « Astronomical Turbulence » in Stellar Atmospheres.

### Summary-Introduction.

A. B. UNDERHILL

*Dominion Astrophysical Observatory*

#### 1. — Introduction.

The term « astronomical turbulence » may be interpreted in a variety of ways. For the purposes of this paper, it is assumed that this expression is equivalent to the term « velocity fields ». Thus this paper will summarize our knowledge of (i) the magnitudes of the velocities occurring in the atmospheres of reasonably stable stars, (ii) the time-dependence, and (iii) the directional characteristics of the velocity fields that exist. Neither motions in the atmospheres of explosive variables, nor motions in the atmospheres of strictly periodic variables, nor solar phenomena will be discussed. This summary is restricted to spectrographic observations of stars that have not changed their character radically since they were first observed some fifty or sixty years ago. The stars which are studied can hardly all be classified as *stable* stars, for changes do occur in their light and spectrum, but in no case are the changes catastrophic.

The terminology used to describe the effects of motion in the stellar atmosphere on the stellar spectrum has grown up in a haphazard manner. This paper attempts to present a unified view of the relevant observations. On the whole, the unsatisfactory current terminology is used so that contact may be retained with the astrophysical literature from which the details are drawn. It is doubtful whether any of the velocity fields inferred here and called « turbulence » closely resemble the phenomenon known as turbulence in wind tunnels.

It has become customary to state that the observations give information about the *microscopic turbulence* and the *macroscopic turbulence* in stellar atmospheres. This division is essentially a result of the methods of analysis that are used.



The *microscopic turbulent velocity* results from curve-of-growth analysis. It is mathematically similar to the most probable velocity of the atoms due to thermal motion. Thus the microscopic turbulent velocity field is assumed to obey a Gaussian distribution law. The exact meaning of the microscopic turbulent velocity is not clear, though it is a quantity that may be obtained by simple analytical methods. Often when stellar spectra are analysed, it turns out that the equivalent widths of the spectral lines obey a curve of growth corresponding to a greater «thermal» velocity than that appropriate to the temperature indicated by the degree of excitation to the various levels in the atom. The difference between the observed velocity and that corresponding to the excitation temperature is called microscopic turbulence.

The term *macroscopic turbulence* covers all irregular, non-periodic motions of the atoms in a stellar atmosphere which are detected by means other than the curve of growth. The macroscopic velocity field is detected from the shapes of the spectral lines, from occasional line doubling, and from the occasional displacement of spectral lines. The motions detected in these ways remain sensibly the same over regions in the stellar atmosphere which are large with respect to the distance in which an absorption line is effectively formed.

One of the greatest problems in the study of motions in stellar atmospheres is to find sensitive and significant means of observing these motions in adequate detail. In order to interpret the results, it is essential that one appreciate how the observations are made and what observations may be made.

In principle one may measure the following three quantities on any spectrogram: (i) the relative intensities of the absorption lines in the spectrum (ii) the shapes of the absorption and emission lines, and (iii) the displacements of the spectral lines. In practice meaningful information can be obtained only by strict attention to detail. All data reported in this paper have been obtained by photographic spectrophotometry.

1'1. *The relative intensity of absorption lines in a stellar spectrum.* — This type of data is fairly easy to obtain. The spectrographic observations must be made with sufficient spectral purity and resolution to isolate the lines being studied from the, often many, other lines in the spectrum, and reliable spectrophotometric techniques must be used. Experience has shown that a linear dispersion of at least  $10 \text{ \AA/mm}$  is required; a dispersion of the order of  $4 \text{ \AA/mm}$  is necessary for many purpose. All intensity measurements are relative to the apparent continuous spectrum in the neighbourhood of the line, the intensity of a line being expressed as an *equivalent width*. Definition of the apparent continuous spectrum is a subtle problem, for in most stellar spectra the absorption lines are very numerous and often overlap.

When the spectral lines are broadened, for instance by motions of the stellar atmosphere, it becomes even more difficult to decide upon the level of the continuous spectrum. Very weak lines usually cannot be studied in detail.

1'2. *The shapes of the absorption and emission lines.* — It is only feasible to make measurements of the shapes of the spectral lines when the lines are somewhat broadened. Thus it turns out that line shapes are a source of useful information only for a few bright stars which have intrinsically wide lines of moderate depth. If the spectral lines are very wide and very shallow little useful information can be obtained because even in the best cases the photographic grain causes random fluctuations in the intensity profile of the order of three to five per cent of the continuous spectrum. Thus if the feature is only 10 to 15 per cent deep at most, the fractional uncertainty in the profile due to grain is large. In principle photoelectric spectral scanning would reduce the uncertainties due to photographic grain, but other practical difficulties may be encountered due to the faintness of the stars. At present, high-dispersion photoelectric scanning of stellar spectra is in its infancy.

1'3. *The displacements of the spectral lines.* — Measurement of the apparent wavelengths of the spectral lines determines a radial, or line-of-sight, velocity. If one knows something about the line-of-sight motion of the star, one can sometimes separate out motions occurring only in the atmosphere from the motion of the star as a whole. In order to investigate atmospheric motions one desires velocities with an uncertainty of 1 to 2 km/s at most. This means determining the positions of the spectral lines (blue-violet region) to about  $0.02 \text{ \AA}$ . At a linear dispersion of  $10 \text{ \AA/mm}$  it follows that the positions of the spectral lines must be determined to within about  $0.002 \text{ mm}$  on the plate. Such accuracy requires excellent techniques of measurement and well defined stellar features. At higher dispersion, it is easier to determine velocity changes of 1 or 2 km/s, for the linear scale on the spectrogram is larger.

In summary it may be said that information about motions in stellar atmospheres can be obtained from stellar spectra with a linear dispersion of  $10 \text{ \AA/mm}$  or better, and that the spectral purity of the spectrograms must be good. Since only few observatories have spectrographic equipment of the necessary power, the observational data to be summarized have been obtained by only a few astronomers. It is unlikely that this body of data will be increased rapidly because each piece of information is the result of much painstaking labour and few astronomers are engaged in this type of work at present.

Before reviewing in detail the data that exist, let us consider the factors limiting further progress. On the theoretical side there are difficulties of inter-

pretation. Since the stars are at great distances, we cannot observe their disks, and the light that reaches us is a weighted mean of the radiation emerging at all angles to the surface and from various depths in the atmosphere. Theories of the stellar spectrum attempt to tell how the observations should be inverted to yield information about the stellar atmosphere, but the theories are in many respects quite rudimentary. In a few special cases, the  $\zeta$  *Aurigae* or *31 Cygni* stars, we can partially resolve the angular problem. In addition much purely physical knowledge about the interaction between radiation and atoms under stellar conditions is lacking. Our knowledge of  $f$ -values, and the shape of the line absorption coefficient under conditions of collisional and radiation broadening is not so complete as is necessary. This lack makes some interpretations uncertain and contradictory.

On the practical side, the faintness of the stars is a severe limitation. We have seen that good spectral purity and a linear dispersion of at least  $10 \text{ \AA/mm}$  is required to obtain useful information about the motions in stellar atmospheres. This means that the stars must be observed with a large reflecting telescope and a powerful spectrograph. Except for the brightest stars the exposure time can quite easily amount to 6 or 8 h. Often a compromise is struck by opening the slit of the spectrograph fairly wide. This procedure, however, may throw away useful information. Furthermore a number of interesting bright stars are not accessible from the northern hemisphere where all active powerful spectrographs are located at present. Most stars have broad ill-defined spectral lines. Since it is only worthwhile to study stars with sharp lines at high dispersion, we are limited to a few objects, not always those that would appear to be most profitable for study.

Another limiting factor is the earth's atmosphere. This blanket permits observations only in certain wave-length regions and frequently only on isolated nights or parts of nights. The interstellar medium also can make the observation of distant objects difficult by dimming the blue-violet light and by impressing broad, ill-defined absorption lines over certain spectral regions that are of interest.

Investigations of the time-dependence of the velocity fields in stellar atmospheres are limited in two ways. 1) Phenomena that occur in periods of less than one or two hours cannot be investigated in detail at high dispersion because, unless the star is very bright, the exposure time to obtain a spectrogram will be longer than one hour. 2) Changes that occur in periods of 100 to 1000 days cannot be investigated fully because frequently the necessary spectrograms were not obtained in the past. There is no way of turning back the clock to study phases that were missed, and there is equally no way of speeding up the clock to study the sequence of events at a rate faster than that at which they actually occur. Practically, the time-limitation is a powerful factor in determining what observations are made.

## 2. — Microscopic turbulence.

STROVE and ELDER (1934) in an important paper showed that one could obtain an estimate of the motions in stellar atmospheres from an analysis of the equivalent widths (integrated absorption) of spectral lines. They showed that a velocity field in the stellar atmosphere would change the shape of the curve of growth, effectively prolonging the portion for which the equivalent width of the line varies directly as the abundance and lifting the so-called transition part of the curve of growth. The apparent result of increased, randomly directed velocities of the atoms on stellar spectra is to make the intrinsically strong lines more intense relative to the intrinsically weak lines. The results obtained by STROVE and ELDER are consistent with later work. Only the results concerning *17 Leporis* should be considered with reserve. STROVE and ELDER realized that *17 Leporis* is a shell star. Further observations have shown that the basic picture of a stellar atmosphere that underlies the method of curve-of-growth analysis does not describe the atmosphere of this star well.

Turbulent velocities have been derived by curve-of-growth methods for some 100 stars. A portion of this data has been summarized by WRIGHT (1955a). Further data may be found in the literature, particularly in papers concerned with abundances of the elements. The results to date are summarized in Table I.

TABLE I. — *Velocities from curve-of-growth analysis.*

No. of stars	Type of object	Microscopic turbulent velocity (km/s)
24	Population I: luminosity classes IV and V	1 ÷ 3
43	" luminosity classes II and III	2 ÷ 6
25	" luminosity classes Ia and Ib	2 ÷ 20
14	Metallic line stars	4
2	Magnetic stars	2 ÷ 5
4	<i>T Tauri</i> stars	3 ÷ 4
2	Subdwarfs	1
5	Population II stars	4

At this point a few words should perhaps be interpolated to inform the non-astronomer on the terminology of Table I. Stars are classified according to the general appearance of their spectra (that is according to spectral type), and according to other properties such as their position in space and their space motion. Population I stars are the common stars which make up the



spiral arms in the vicinity of the sun. Population II stars are stars having a radically different space motion. They are believed to compose the bulge of the galaxy and to be scattered thinly around the galaxy in a halo. Much evidence suggests that these stars are older than Population I stars. The metallic line stars are stars in which the absorption lines from the ionized and neutral metals are unusually strong in comparison to the hydrogen lines. Magnetic stars are stars which are known to possess strong magnetic fields. No test for magnetic field is known that can be used for all stars, thus there is no information about the presence or absence of a magnetic field for most stars. The *T Tauri* stars are comparatively cool, irregularly variable stars which have a spectrum containing emission lines like those of *T Tauri*. It is suspected that these stars are very young, and that they are still in the contracting stage. Subdwarfs are stars about one magnitude fainter than normal stars of the same spectral type.

A system of spectral classification (see, for instance, KEENAN and MORGAN, 1951) has been developed for the common, Population I stars. Each gross type is denoted by a letter. The gross types are subdivided into sometimes as many as ten subtypes which are indicated thus: B1, B2, B5. The spectral type classification is essentially a classification according to temperature, the spectral classes being arranged as in the following tabulation. The B1 stars are hotter than the B2 stars, the B2 stars hotter than the B5 stars, and so on. The average speed of a hydrogen atom at these various

Spectral type	Approximate temperature in the atmosphere (°K)	Average velocity of H atoms (km/s)
O	28000 to 40000	21.4 to 25.7
B	11800 to 25000	14.0 to 20.3
A	8100 to 11000	11.6 to 13.5
F	6000 to 7600	10.0 to 11.2
G	5000 to 5800	9.1 to 9.8
K	4000 to 5000	8.1 to 9.1
M	2600 to 3600	6.6 to 7.7

temperatures is listed as a convenient reference with which to compare the velocities of Table I. Stellar atmospheres are composed predominantly of hydrogen.

When a group of stars of a given spectral type is examined, it is found that the stars do not all have the same absolute brightness. Because such stars all have about the same surface temperature, this variation in intrinsic brightness (which may be anything from one magnitude to over ten magni-

tudes) is essentially due to differing size. Hence the origin of the terms subdwarf, dwarf, giant, and supergiant star. This spread in size is reflected by a considerable range in pressure in the stellar atmospheres. In dwarf stars the pressure is about  $10^4$  dyn/cm<sup>2</sup>, in giants it is about  $10^2$  dyn/cm<sup>2</sup>, and in supergiants it may be about  $10$  dyn/cm<sup>2</sup>. Stars may be separated according to their intrinsic luminosity (size) by carefully observing the relative intensity of certain spectral lines which are pressure sensitive. The Keenan-Morgan system makes use of «Luminosity Classes» to describe this differentiation. The supergiants fall in luminosity classes Ia and Ib, the giants in classes II and III, the dwarfs in classes IV and V. When stars are plotted on the spectral type-absolute luminosity plane, it is found that the points are not randomly distributed, but that they fall along certain well-defined sequences. Such a plot is called a Hertzsprung-Russel diagram (HR diagram) after the discoverers of this phenomenon. The HR diagram of Population I stars is different from that of Population II stars. One may infer that the differences are due to evolution changes (cf. Fig. 1 of DEUTSCH paper, part III-C for an HR diagram).

The significance of the numbers in Table I and specially the significance of the differences between the individual values included in these means are matters that require consideration. In the first place, any systematic errors in the equivalent widths will affect the estimate of the microscopic turbulent velocity directly, because this velocity is determined from the difference between the observed ordinate,  $\log W/\lambda$ , and the theoretical ordinate,  $\log (W/\lambda) \cdot (c/r)$ . It is known that there are sometimes systematic differences between equivalent widths measured on different spectrograms. In part these are due to differences of interpretation of the stellar spectrum, in part they are due to insufficient resolving power of some of the spectrographs used, and in part they are due to faulty calibration of the photographic plates. It is sometimes a delicate task to maintain a calibrating device in proper order, and it often takes an experienced worker to realize that something is wrong. Unfortunately sufficient attention has not always been paid to the question of whether the calibration is good or not. Very few papers devote as much as a sentence or two to the question of whether the measured equivalent widths are what they are supposed to be. Photometric calibrations are rather like the little girl in the rhyme:

When they are good, they are very, very good,  
But when they are bad, they are horrid.

Secondly, the shape of the theoretical curve of growth to which the observations are fitted is a function of the model used. Consequently, because there is a choice of models, there is a small intrinsic scatter in the value of the parameter  $r$  that may be derived from a given set of data.

Thirdly, the observed points frequently do not define well a single curve of growth owing to uncertainties in the theoretical abscissa ( $\log gf$  values), and to random errors in the measured equivalent widths. Usually the uncertainty in the derived value of  $\log v$  is at least  $\pm 0.1$ . The uncertainty of fit may be larger in cases such as the supergiants where it is quite doubtful whether any of the models underlying the method of analysis is applicable.

HUANG (1950, 1952) has considered some of these points from the theoretical point of view. He concludes that unless the real distribution function of the eddy velocities is known, the meaning of the turbulent velocity derived from the curve of growth is uncertain. Nevertheless, it seems probable that the parameter  $v$  does give an order of magnitude estimate of the most probable velocity differences between atoms in the line of sight.

The following generalizations appear to be secure.

1) Motions in stellar atmospheres increase as the luminosity of the stars increases. In the supergiants the microscopic turbulent velocity correlates with the excitation potential of the lower level from which the line arises. The correlation is in the direction that lines of low excitation potential reflect a larger range of velocity than do lines of high excitation. This correlation was first noticed by WRIGHT (1947). More recent data on supergiant spectra (see for instance ABT, 1958) confirm this trend. HUANG (1952) has shown that this behaviour is consistent with the assumption

$$v_{\text{turb}} = v_0 \exp[-\alpha\tau]$$

where  $\tau$  is the optical depth in the continuous spectrum and  $v_0$  and  $\alpha$  are constants which vary from star to star. Since lines of high excitation are usually formed at deeper levels in the atmosphere than lines of low excitation, this relation suggests that turbulent velocities are less in deep layers.

2) The metallic line stars, peculiar A stars, and magnetic A stars appear to show a somewhat larger microscopic turbulence than do normal stars of similar luminosity. E. M. BURBIDGE and G. R. BURBIDGE (1955) have investigated how much of this increase might be due to magnetic effects. They conclude that the magnetic broadening would contribute about 1 km/s.

3) The *T Tauri* stars (BONSACK and GREENSTEIN, 1960) and the Population II stars (BURBIDGE and BURBIDGE, 1956) have rather greater microscopic turbulence than main sequence stars of roughly similar spectral types.

4) The subdwarfs show very little microscopic turbulence.

HUANG and STRUVE (1952, 1955) using the concept of large and small eddies, have introduced methods of analysing high dispersion spectra that attempt to take into account the fact that microscopic turbulence chiefly

affects the equivalent width of a line while macroscopic turbulence affects chiefly the line shape. They note that if one plots the apparent half-width of the absorption-line profiles as  $\log D_\lambda/\lambda$  (where  $D_\lambda$  is the half-width in wavelength units) against the equivalent width as  $\log W/\lambda$ , one obtains a curve of characteristic form. From the displacements necessary in both co-ordinates to fit the theoretical curve to the observed curve, one may estimate the most probable velocity of small eddies and the most probable velocity of large eddies. They also show that the slope near the origin of a plot of line depth against  $W/\lambda$  gives information on the contribution from large and small eddies.

These methods have been applied by HUANG and STRUVE (1952, 1953, 1955) to spectra of the supergiants  $\delta$  *Canis Majoris* (F8Ia),  $\alpha$  *Cygni* (A2Ia), and  $\rho$  *Leonis* (B1Ib); by WEHLAU (1956) to spectra of the double star  $\gamma$  *Leonis*, both components of which are giant stars; and by MICZAIKA and WADE (1958) to spectra of the metallic line star  $\delta$  *Comae*. In each case it turns out that the most probable velocity of the small eddies is close to the value found from curve-of-growth studies, and that the most probable velocity of the large eddies is not much larger. In the case of  $\delta$  *Canis Majoris* the relationship between excitation potential and microscopic turbulent velocity which is found by curve-of-growth analysis is reproduced by these methods.

The methods of line-width and line-depth correlation can only be used with spectrograms of the highest dispersion and resolution (about 3 Å/mm) and then only for stars with intrinsically wide lines.

### 3. - Macroscopic turbulence.

3.1. *Generalities.* - The concept of macroscopic turbulence has grown up in the last 15 years to explain detail observed on many high-dispersion spectrograms. However, since few observing programs have been directed towards investigating macroscopic turbulence in detail, the relevant material must be extracted from the results of other investigations. Because of the chance distribution of observations, one cannot investigate the time-dependence of macroscopic turbulence in as much detail as would seem desirable. In what follows, the term macroscopic turbulence is taken to mean all irregular, non-periodic motions in stellar atmospheres that occur in bodies of gas large with respect to the distance in which an absorption line is formed, that is, all motions which affect the line shape and position rather than the line strength and which change from time to time.

I must apologise to the non-astronomers present for speaking during the next few minutes in detail about a few selected stars. Only in this way can I put the data before you. Each star has a character of its own, and the available information about each illuminates some facet of the problem



before us. I have selected stars for which considerable data (much of it yet unpublished) were available to me at the Dominion Astrophysical Observatory. Other persons would probably put other samples before you. However, I hope what I do present will give a fair sample of the fields of velocity which exist in some stellar atmospheres. Two things will become clear. (i) Large velocities can and do exist in the envelopes of low density gas around stars. (ii) These velocities change from time to time. The stars which are discussed have conspicuous envelopes and conspicuous macroturbulence. Little information can yet be deduced about cases where macroturbulence is not prominent.

How are the motions of atoms in different parts of a stellar atmosphere observed? In the first place only the line-of-sight or *radial* velocity component can be measured. This is found by the first order Doppler effect. The transverse Doppler effect gives displacements that are too small to be detected in stellar spectra. Since, with stars, one receives the light from all directions in one spectrogram, the possibility of separating out certain components of velocity depends upon the relative sizes of the *intrinsic* range of velocities which exists at the moment and the *effective* range introduced by limb-darkening and the projection factor. If there is a very skew distribution of velocity in the atmosphere, the spectral lines may become asymmetrical, shaded to the red or the violet depending upon the direction of the velocity field, or the lines may become double or even triple. Generally the velocity distribution is only sufficient to broaden the line in a symmetrical manner. Occasionally asymmetrical spectral lines are observed.

An estimate of the magnitude of the velocities occurring can be made by measuring the width and shape of the line profiles, and by measuring the displacement of the spectral lines. In order to conclude that a line is displaced, one must first know what the apparent wave-length should be when the line is not displaced. Since all stars are moving in space, and since one does not usually know the stellar velocity *a priori*, it is not always clear what the apparent wave-length of the undisplaced line should be. When the star is a member of a binary system, one can compute the line-of-sight motion of the star once the orbit is known. Then one can compute the apparent wave-length of each spectral line at any phase. A similar case is when the star is known to belong to a moving cluster or stream. Then one can assume, with reasonable confidence, that the motion of the star is known. Any apparent departure from this value can be credited to atmospheric motions. A weaker condition yet, is to assume that the star has a constant velocity. Then any apparent fluctuation in this velocity may be attributed to atmospheric motions. It is difficult at times to decide between perturbations in the star's motion due to membership in a binary system and fluctuations in radial velocity due to atmospheric motions.

Ordinary main-sequence stars do not have *observable* extended atmospheres. We shall be concerned chiefly with supergiant stars and early-type stars which are surrounded by extended atmospheres or shells. It is wise to recall that if we could only observe the integrated light from the sun, for example as it is reflected from the moon, we would know very little about motions in the outer atmosphere of the sun. There is not enough material in the chromosphere and the corona for these parts of the solar atmosphere to contribute much to the integrated spectrum. The same is true for most main-sequence stars. Our knowledge of motions in the atmospheres of stars is severely limited by the means of observation available to us.

In general information about motions in stellar atmospheres will be given by the intrinsically strong absorption or emission lines from abundant elements. Roughly speaking an absorption line is not visible unless there is sufficient material in the line of sight to produce an optical depth of the order of unity. Since optical depth is measured by the product of the number of atoms in the line of sight capable of absorbing the line and the atomic absorption coefficient, it is clear that fewer atoms will form an observable feature when the line is intrinsically strong than when the line is weak. In order to investigate fully the velocity fields in stellar atmospheres we should like to be able to observe relatively small volumes of gas. This can only be done by means of very strong spectral lines.

It is certain that conditions of strict local thermodynamic equilibrium do not exist in all parts of the extended atmospheres where macroscopic turbulence occurs. However, not enough is known about the interactions between radiation and atoms that occur to make it worth-while to apply any detailed theory of line formation at present. Rather it is simply assumed that the line profile mimics the shape of the absorption coefficient and that line displacements relate directly to velocities. When the theory of line formation (absorption and emission) is sufficiently well understood, it may be worth-while to go over the observations and sharpen the interpretation.

Suitable spectral lines for revealing macroscopic turbulence in stellar atmospheres are the following:

*in absorption:*

- late-type stars: resonance lines, particularly of Ca II and Ca I, strong lines from levels of low excitation in the spectra of Fe I, Mn I, Cr I and Ti II chiefly;
- early-type stars: Balmer lines of hydrogen, and lines arising from metastable levels. These include certain He I lines and many lines of Fe II, Cr II, etc.;

*in emission:*

- late-type stars: resonance lines of Ca II, selectively excited lines of low excitation, Balmer lines of hydrogen;
- early-type stars: Balmer lines of hydrogen, particularly  $H_\alpha$ , selectively excited lines such as C III  $\lambda$  5696, N III  $\lambda$  4640, and He II  $\lambda$  4686.

### 3'2. Evidence for macroscopic turbulence in stellar atmospheres.

3'2.1. Supergiants. — Supergiants of all spectral types are readily recognizable by the broad, steep-sided absorption lines in their spectra. The supergiant absorption lines are typically  $\exp[-x^2]$  in shape with a flat bottom (peak). Realization that relatively large motions may occur in the atmospheres of supergiants stems from the observation by STRUVE (1946) that the strong lines in the spectrum of  $\delta$  *Canis Majoris* are wider than the weak lines, and that the velocity spread corresponding to the profiles of the strong lines is significantly greater than that corresponding to the curve of growth. The theoretical implications of these details were discussed by UNSÖLD and STRUVE (1949). Many more observations are now available about the motions of gas in the atmospheres of supergiants and other stars. Some of these data will be presented.

All supergiants that have been observed photometrically over any considerable period are found to vary slightly and irregularly in light. Also, the radial velocity of most, if not all, supergiants varies irregularly by a small amount. Changes in line shape have been recorded for a few supergiants. More changes in line shape might be discovered if the supergiants were observed regularly at very high dispersion (2 to 3 Å/mm). Unfortunately such an observing program is rather impracticable.

Nearly everyone (see for instance HUANG (1953), SLETTEBAK (1956), and ABT (1958)) who has attempted to interpret the line profiles of the supergiants has noted that from the shape of the profiles alone one cannot differentiate between broadening due to rotation with an equatorial velocity of some 25 to 50 km/s and macroturbulence with a most probable velocity of the order of two-thirds the necessary rotation. Further information is required to resolve this difficulty.

The available information about velocities in the atmospheres of supergiants is summarized in Table II. The entries in this Table are explained in the following paragraphs. Unless credit is otherwise given, the material that is presented has been obtained by me from spectrograms taken at the Dominion Astrophysical Observatory by my colleagues and by myself. I am grateful for permission to use the extensive body of material which has been assembled at the Dominion Astrophysical Observatory over the years. What is presented here is a mere skimming of the field.

TABLE II. — *Velocities occurring in the atmospheres of supergiants.*

Type of star	Radial velocity range (km/s)	Shapes of absorption lines (km/s)	Curve of growth (km/s)	$H_{\alpha}$ emission profile (km/s)	Moving shell and or clouds (km/s)	Chief objects studied
B Ia	30	20 ÷ 40	15 ÷ 30?	~ 200	~ 200	$\chi^2$ Ori, 55 Cyg
B Ib	30	20 ÷ 60	15			67 Oph, $\eta$ Leo
A Ia	4 ÷ 20	12 ÷ 15	13	(~ 200)	(~ 100)	$\alpha$ Cyg
A Ib		10 ÷ 20	5 ÷ 7			H.R. 8345, H.R. 2874
F Ia	4 ÷ 20	15 ÷ 30	2 ÷ 26			$\delta$ CMa, $\epsilon$ Aur, 89 Her
F Ib		6 ÷ 20	6			$\alpha$ Per
K Ib	7 ÷ 10	10 ÷ 20	4 ÷ 13		≤ 100	31 Cyg
M Ia	10 ÷ 20				10 ÷ 50	$\alpha$ Ori, VV Cep

a) B-type supergiants. The stars  $\chi^2$  Orionis (B2Ia), 55 Cygni (B3Ia), 67 Ophiuchi (B5Ib), and  $\eta$  Leonis (B1Ib), are representative of the early-type supergiants. All of the Victoria spectrograms of  $\chi^2$  Orionis, 55 Cygni and 67 Ophiuchi that have been obtained with resolving power better than one-prism were measured for radial velocity. The results are summarized in Table III.

TABLE III. — *Summary of radial velocity results for 5 B-type supergiants.*

Star	No. of spectrograms	First observation	Last observation	Range in radial velocity (km/s)	Remarks
$\chi^2$ Ori	13	1938 Dec. 19	1959 Nov. 16	15	( <sup>1</sup> )
$\chi^2$ Ori	74	1921 Feb. 20	1941 Jan. 18	30	( <sup>2</sup> )
55 Cyg	52	1959 Aug. 27	1959 Aug. 6	30	( <sup>3</sup> )
67 Oph	26	1943 July 2	1959 Aug. 6	30	( <sup>4</sup> )
H.D. 14134	18	1924 Sept. 12	1952 Nov. 23	29	( <sup>5</sup> )
H.D. 14143	16	1924 Sept. 12	1952 Nov. 23	33	( <sup>5</sup> )

(<sup>1</sup>) Fluctuations random and large.

(<sup>2</sup>) Mount Wilson observations; large fluctuations.

(<sup>3</sup>) Possible drift to more positive velocities; large fluctuations.

(<sup>4</sup>) Possible velocity peak in 1949.

(<sup>5</sup>) One-prism spectrograms measured by R. M. Petrie; large fluctuations.

The results of a long series of observations of  $\chi^2$  Orionis by R. E. WILSON at the Mount Wilson Observatory confirm and expand the Victoria results. WILSON published his material only in summary form (WILSON, 1953), but the manuscript results were most kindly forwarded to me from the Mount Wilson observatory. No attempt has been made to confirm whether or not



WILSON used the same wave-lengths as those adopted here. For the present purpose the exact wave-lengths used are immaterial — all that is required is that the *same* wave-lengths be used for all spectrograms of a series. Also some observations due to R. M. PETRIE of two of the B-type supergiants in *h* and  $\chi$  *Persei* are included.

The radial velocity of each of these stars fluctuates in an irregular manner, the range being greater than can readily be accounted for by the uncertainties of measurement. The observations are too scattered to rule out a regular, small-amplitude variation with a period of 10 to 20 days, but such a periodic variation does not seem probable. A survey of all available data for  $\rho$  *Leonis* would likely give similar results. These radial-velocity fluctuations are most probably due to atmospheric motions.

Conspicuous changes of line shape have not been observed in the blue-violet part of the spectrum of any of these B-type supergiants. However,

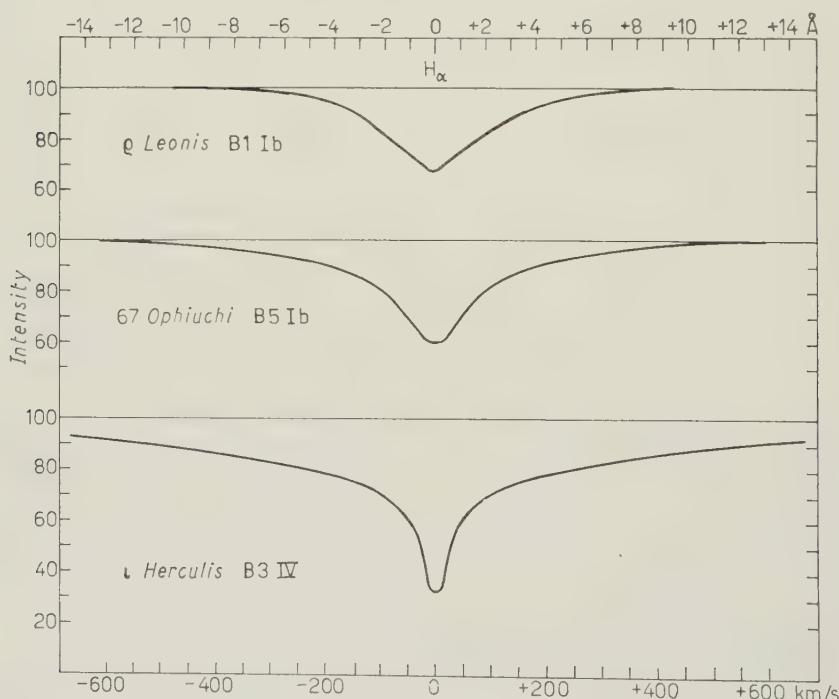


Fig. 1. — Profiles of  $H_{\alpha}$  in the spectra of the B-type supergiants  $\rho$  *Leonis* and 67 *Ophiuchi*, and in the spectrum of the B-type main-sequence star *L Herculis*.

the profile of  $H_{\alpha}$  changes from time to time in the case of the Ia supergiants  $\chi^2$  *Orionis* and 55 *Cygni*. The changes are shown in Figs. 1, 2, and 3, which contain all the data available at the Dominion Astrophysical Obser-

vatory. These line profiles have been smoothed, and although the spectral purity (indicated by the length of the projected slit image) is not so great as might be desired, the profiles do give an impression of the changes that

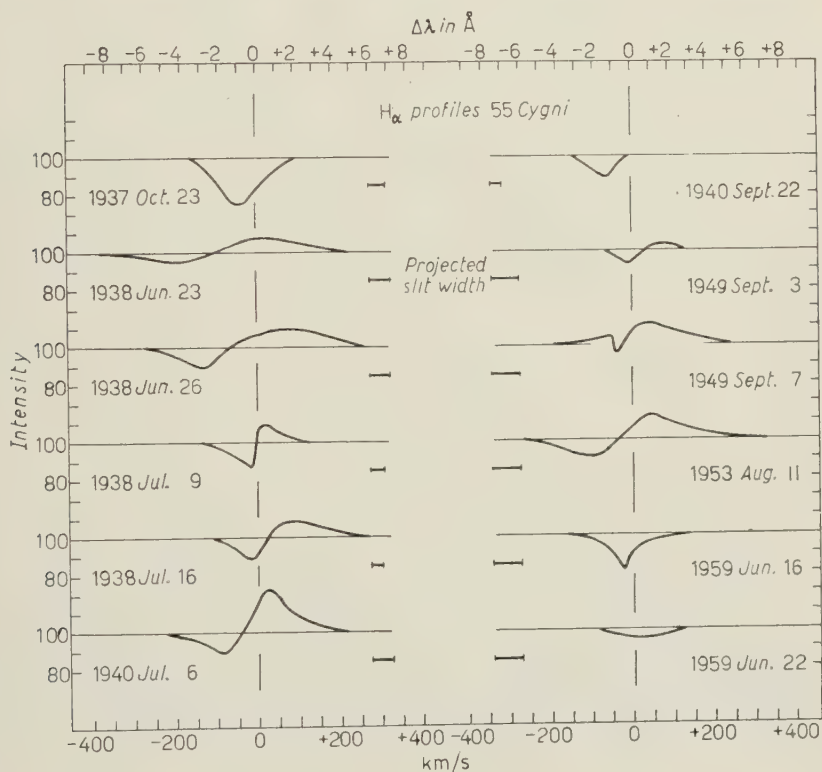


Fig. 2. — Profiles of  $H_{\alpha}$  in the spectrum of the supergiant *55 Cygni* at various dates. It is clear that the emission intensity varies.

occur at  $H_{\alpha}$ . Detail which is only as wide as the slit image, is by no means certain. The ups and downs have been traced as it seemed to me they appeared on the spectrograms; but the photographic grain is bad. The position of the undisplaced wave-length of  $H_{\alpha}$  was determined by measuring the appropriate distance on the tracings from the positions of the C II lines  $\lambda 6578$  and  $\lambda 6582$  which are fairly well defined.

Fig. 1 displays the purely absorption profile of  $H_{\alpha}$  in *67 Ophiuchi* (B5Ib) and in  *$\rho$  Leonis* (B1Ib). These line profiles are what one would expect a normal stellar reversing layer to produce when the density is low, as in a supergiant. For comparison, the profile of  $H_{\alpha}$  in  *$\iota$  Herculis*, a B3 main-sequence star, is also shown. Here the effect of Stark broadening is evident. These line profiles have not changed by a detectable amount during the

period of observation, and the position of the centre of the absorption dip agrees closely with the position projected from the position of the neighbouring C II lines.

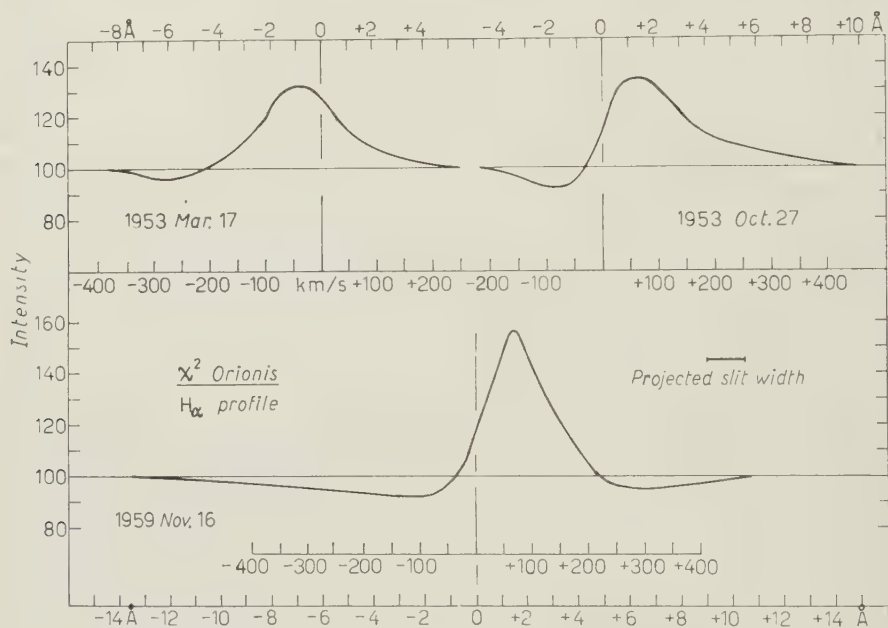


Fig. 3. — Profiles of  $H_{\alpha}$  in the spectrum of the supergiant  $\chi^2$  Orionis. The shape and the intensity of this line varies from time to time. The projected slit width is  $1.5 \text{ \AA}$  in each case.

Fig. 2 displays the profile of  $H_{\alpha}$  in  $\zeta^5$  Cygni at various dates. At times only a weak absorption feature is visible; at times there is emission accompanied by violet-displaced absorption, the emission varying in strength and shape; at times there is no distinct feature either in emission or in absorption. The emission wings may extend  $200 \text{ km/s}$  from the centre of the line.

Fig. 3 shows the profile of  $H_{\alpha}$  in  $\chi^2$  Orionis upon three occasions. The emission is stronger than in  $\zeta^5$  Cygni, and the emission wings extend to at least  $200 \text{ km/s}$  from the centre of the line. The shape and strength of the emission is variable. It is quite noticeable that the position of the most intense part of the emission feature shifts.

These data are barely reliable enough to do more than indicate the large changes that occur in the velocity of the gas and in the quantity of gas in the envelopes around these stars. There is no doubt that the  $H_{\alpha}$  emission is more than  $200 \text{ km/s}$  wide at times in  $\zeta^5$  Cygni and in  $\chi^2$  Orionis. At other times it has nearly vanished. These changes in the shape and strength of

the  $H_\alpha$  line in the very luminous stars are almost certainly due to changes in the velocity and density of the gas around these stars. They are not due to Stark effect. It is quite possible that corresponding changes might be detected in the strong absorption lines of the blue-violet spectral region were spectrograms of sufficiently high resolution and spectral purity obtained. It is a significant point that the  $H_\alpha$  line appears to be a normal absorption line constant in shape in the Ib supergiants 67 *Ophiuchi* and  $\rho$  *Leonis*. An obvious inference is that the extremely luminous supergiants are surrounded by larger envelopes of moving gas than are the less luminous supergiants. Since the absorption component which accompanies the emission feature in the Ia stars is displaced to the violet, it would seem that on the average the gas is moving away from the stellar surface. The emitting volume must be quite large, for the emission fills in almost completely the underlying absorption  $H_\alpha$  profile which would be expected to originate in the stellar reversing layer proper. A hint of this underlying line is seen on the spectrogram of  $\chi^2$  *Orionis* obtained November 16, 1959.

VOIGT (1952) and ALLER (1956) have attempted to analyse the spectrum of 55 *Cygni* by curve-of-growth and line-profile methods. ALLER concludes that the curve of growth is inconclusive on turbulent velocity, and that perhaps a macroscopic turbulence of 18 km/s would represent the line profiles better than the value of 36 km/s suggested by VOIGT. All attempts that have been made to interpret the line profiles in the blue-violet part of the spectrum of B-type stars of luminosity classes II, Ib, and Ia have indicated that velocities of the order of 20 to 30 km/s at least occur in the atmospheres of these stars. The  $H_\alpha$  profiles in the spectra of the Ia stars make visible much larger velocities. GOLDBERG (1939) has made curve-of-growth studies of a number of B stars.

b) A-type supergiants. The star  $\alpha$  *Cygni* (A2Ia) is the A-type supergiant which has been studied most, for it is apparently bright. The radial velocity of this star varies. PADDOCK (1935) at the Lick Observatory obtained many spectrograms in the years 1928-31, and he has published the results in detail. The note in the *Lick Radial Velocity Catalogue* (MOORE, 1932) summarizes the situation succinctly: «The velocities vary from  $\pm 6$  km/s to  $-9$  km/s in the interval, but the character of the variation is such that it would be misleading to attempt to assign an adopted value for the velocity of this star». This would be a suitable description of the radial velocity results reported above for five B-type supergiants. The maximum range of the velocity variation of  $\alpha$  *Cygni* is about 15 km/s. In addition  $\alpha$  *Cygni* is known to be a light variable of small range (for references, see ABT, 1957). STRUVE and HUANG (1955) have studied line shapes in the spectrum of  $\alpha$  *Cygni* from one spectrogram, and they estimate that the most probable velocity



of the small eddies is about 15 km/s, whereas that of the large eddies is greater than or equal to 12 km/s. Curve-of-growth studies (BUSCOMBE, 1951) give  $v$  equal to 13 km/s.

The most extensive investigation of A-type supergiants is that by ABT (1957, 1958). In 1956 ABT took spectrograms of 6 Ia supergiants every night for one month. He found that each varied in velocity, the range lying between 4 and 20 km/s. ABT inclined to the view that the variations were periodic, the characteristic period being about 7 to 9 days. It is doubtful whether extended observations over several years would confirm this periodicity. It is more probable that the radial velocity of each supergiant would continue to vary irregularly with a range somewhat as found by ABT. These variations are probably due to atmospheric motions, for ABT finds differences in the velocity derived from lines of different ionic spectra and from hydrogen. The ranges exhibited by the A-type supergiants are slightly smaller than those found for the B-type supergiants. ABT (1958) gives information about line shapes in the spectra of some A Ib and F Ib supergiants.

BEALS (1951) has noted that emission appears at  $H_x$  in the spectrum of  $\alpha$  Cygni accompanied by a violet-displaced absorption feature. The emission feature seems to be variable in intensity, but no quantitative data exist at present. An inspection of the Victoria spectrograms gives the impression that the emission feature is quite wide, the emission perhaps extending 100 km/s from the undisplaced centre of the line.

c) F and G-type supergiants. The supergiant  $\delta$  Canis Majoris (F8 Ia) is the first star for which the effects of macroscopic turbulence were recognized. STRUVE (1946) noted that the profiles of the strongest lines had shapes suggesting a most probable velocity near 30 km/s, while the curve of growth is consistent only with a velocity of the order of 5 km/s. The radial velocity observations of this star (CAMPBELL and MOORE, 1928) indicate a probable slow variation with a range of about 6 km/s. ABT (1957) found no significant variation during the short period of his observations. ABT observed 3 other F-type supergiants for radial-velocity and he finds that radial-velocity fluctuations occur in a matter of days, the range being about 4 km/s.

Much attention has been directed to the F0 Ia supergiant  $\epsilon$  Aurigae. This star is the brighter component of an eclipsing system which has a period of some 27 years. Near conjunction, when the fainter star is moving in front of the F-type supergiant, extra, relatively sharp F-type lines are observed in the spectrum. These components occasionally are double or triple. Although there is no unanimous interpretation of the  $\epsilon$  Aurigae system, quite plausible arguments may be advanced that the companion is a Be star. However, we do not wish to digress upon the fascinating peculiarities of  $\epsilon$  Aurigae. The detailed work on this supergiant has shown the following facts

relevant to the present discussion:

1) Irregular fluctuations, which are probably due to atmospheric motions, occur in the radial velocity of this star. The range is some 10 to 15 km/s and there are indications that lines from different ions give different velocities, that is, that there is a velocity gradient through the atmosphere.

2) The light of  $\epsilon$  Aurigae varies irregularly by about 0.2 mag, quite apart from the light changes due to the eclipse.

3) The line profiles of the F-type supergiant change in shape from time to time. This behaviour was first noticed by WRIGHT (1955*b*) and it is very apparent on Wright's most recent series of high dispersion spectrograms. The most probable cause of these changes is change in the velocity field of the atmosphere.

In order to obtain a quantitative measure of the changes in line shape that occur, and thus to estimate the changes in the velocity field, I measured the profile of Mg II  $\lambda 4481$  on a series of intensity tracings of spectrograms obtained by WRIGHT between November 16, 1956 and May 15, 1957. During this period, the star was coming out of eclipse, and extra components were visible at some lines. However, an extra component does not appear for Mg II  $\lambda 4481$  because the lower level of this line is not metastable. Therefore, the shape of  $\lambda 4481$  reflects the distribution of velocities in the stellar atmosphere and not atmospheric effects associated with the eclipse. The shape of the profile was determined by fitting it to a Voigt profile (VAN DE HULST and REESINCK, 1947). It turns out that the parameter  $\beta_1$  which represents the dispersion part of the profile is small and of little importance. Effectively, the line is fitted to a curve of the form  $\exp[-(\Delta\lambda/\beta_2)^2]$ . The resulting values of  $\beta_2$  are shown in Fig. 4, plotted against time. The parameter  $\beta_2$  is expressed in km/s. It is obvious that  $\beta_2$  varies from over 100 km/s to somewhat less than 70 km/s in a few days. This means that the apparent halfwidth of the profile changes by some 30 km/s in a quite irregular manner. The line at no times has extensive wings. (The Mg II  $\lambda 4481$  line is a natural doublet of 0.2 Å separation and it is not quite resolved on the present spectrograms. The turbulent broadening to which the observations refer is superposed on the natural doubling).

Somewhat similar changes of line shape have been observed for 89 *Her- culis*, an F2Ia supergiant (BÖHM-VITENSE, 1956). The light (WORLEY, 1956) and radial velocity of this star vary, the range of the radial velocity being 20 km/s.

Another very luminous star,  $\rho$  Cassiopeiae, shows related spectral changes (BIDELMAN and McKELLAR, 1957). The light of this star has recently changed by two magnitudes, and the spectral type has changed from F8Ia to some-

thing like G8Ia. At present many of the lines of this star are double, the extra component being displaced some 35 km/s to the violet of the component due to the normal reversing layer. Presumably this star is ejecting material.

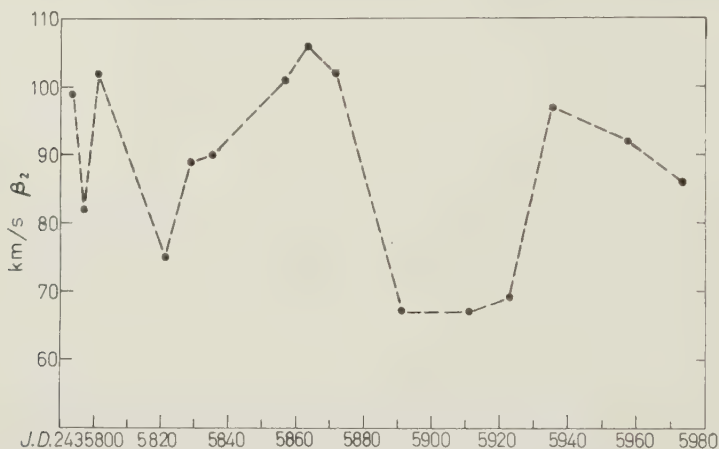


Fig. 4. — The variation of line width in the spectrum of  $\epsilon$  Aurigae. The parameter  $\beta_2$  measures the apparent width of the unresolved doublet Mg II  $\lambda$  4481.

for considerable evidence pointing to an extensive shell exists. At all times the spectral lines of  $\rho$  Cassiopeiae have the appearance considered to be characteristic of large «macroturbulence».

d) K-type supergiants. The K-type supergiant for which we have the most detailed knowledge of atmospheric phenomena is 31 Cygni, K3Ib. This star is the primary component of an eclipsing system, the secondary of which is a B3V star. This system is particularly valuable because, as the B star passes behind the K star, the light from the B star probes the extensive supergiant atmosphere. A sketch of the system is shown in Fig. 5.



Fig. 5. — The eclipsing system 31 Cygni. The extended atmosphere of the K-type supergiant is shown as a shaded area. The disk of the K star is drawn as a black circle. The B-type star is shown by a small dot. Chromospheric absorption lines are observed as the B star moves behind the atmosphere of the K star.

Extra absorption lines appear in the composite spectrum as totality approaches, the strengths and shapes of which reflect the properties of that part of the

K-type atmosphere which is projected on the disk of the B star. Analysis of the displacements, shapes, and intensities of these so-called chromospheric lines gives some indication of the physical properties of the K-type atmosphere, particularly of the variation of the density and velocity fields with height above the limb of the K star. The complete atmospheric eclipse takes some 80 days, the daily motion of the B star being  $3.87 \cdot 10^6$  km. Thus *31 Cygni* is one case where a fairly reliable *linear* scale may be inferred for a supergiant atmosphere. Extensive observations have been made at the Dominion Astrophysical Observatory and elsewhere; detailed accounts of the system are given by MCKELLAR and PETRIE (1959), MCKELLAR, ALLER, ODGERS, and RICHARDSON (1959), WRIGHT and LEE (1959), and WRIGHT (1959).

When the B star begins to go behind the atmosphere of the K star, the intrinsically strong *H* and *K* lines of Ca II form the first chromospheric components to appear. These absorption lines strengthen as the projected position of the B star gets closer to the limb of the K star. At times several absorption components due to Ca II *H* and *K* are seen. These absorption lines provide unmistakable evidence that bodies of gas moving with discrete velocities exist in supergiant atmospheres. These «clouds» are observed to have relative velocities in the line of sight of up to 100 km/s, and the dimensions of the clouds are about  $10^7$  km. Near the limb of the K star, at about 3 days or  $12 \cdot 10^6$  km out, the density of the K-type atmosphere suddenly increases and many lines of Fe I, Ti II, etc. appear as chromospheric components.

The apparent wave-lengths of the Ca II chromospheric lines indicate erratic departures from the orbital radial velocity. These departures are probably due to atmospheric motions. The range is 7 km/s.

The shapes of the line profiles of the chromospheric components may be fitted best by a most probable turbulent velocity of 20 km/s in the inner chromosphere, and by a most probable turbulent velocity of 10 km/s in the outer chromosphere.

WRIGHT (1959) has made curve-of-growth analyses of the chromospheric spectrum. He derives microscopic turbulent velocities of 4 to 13 km/s from lines of Ca I, Sc II, Ti I, Ti II, Fe I, Co I, and Ni I. The results are effectively the same for neutral and ionized atoms. The results are most comprehensive for Fe I where the curve of growth indicates that the microscopic turbulent velocity is about 7 to 8 km/s in the inner chromosphere (within 3 days of totality), but beyond 5 days the value increases to 12 km/s. This analysis gives an «excitation temperature» increasing outwards from 2750 at the limb to  $5500^\circ$  at 18.7 days out ( $69 \cdot 10^6$  km).

Faint emission lines due to Ca II *H* and *K* appear in the bottoms of the strong stellar Ca II *H* and *K* absorption lines in the spectra of most, if not all, G and K-type stars. O. C. WILSON and VAINU BAPPU (1956) made the



first systematic observations of these features. They noted that the apparent width of the emission lines correlates directly with luminosity. The emission features are widest in the most luminous stars. Presumably this correlation, which affords a convenient, empirical means of estimating the luminosity of G- and K-type stars, is a manifestation of the velocity fields present in G- and K-type atmospheres.

e) *M-type supergiants.* Supergiant M-type stars have extremely extended atmospheres. It is not unusual for the diameter of an M-type supergiant to be one thousand times that of the sun. Consequently, the gravitational control of the gas in these atmospheres is small, and large scale, irregular motions can develop. The radial velocities of a few M-type supergiants have been observed in detail, and in each case large random irregularities occur, the range being about 10 to 20 km/s. The line profiles are broad and steep-sided. On spectrograms of the highest dispersion many spectral lines appear double. Part of the doubling is due to the occurrence of emission components in the centres of strong lines, but part seems to be due to atmospheric motions. In addition, certain sharp, displaced absorption lines appear. These lines may be attributed to absorption in a very low density shell of gas surrounding the whole system and expanding from the star.

High-dispersion spectrograms of the eclipsing system *UV Cephei*, which consists of an M2 supergiant and Be star, and which forms a system geometrically similar to the *31 Cygni* system, reveal detail generally similar to that observed for *31 Cygni*. Thus, extra chromospheric lines are observed as the Be star begins to go behind the vast M-type atmosphere. These chromospheric lines indicate that discrete clouds probably occur in the M-type atmosphere and that these clouds may have relative velocities as high as 50 km/s. Also a circum-system envelope expanding at a rate of about 20 km/s is observed. The period of the *UV Cephei* system is 20.3 years and totality lasts some 450 days. Spectroscopic effects as the Be star passes behind the M-type atmosphere are visible at least a year before and a year after totality. The total phases of the latest eclipse commenced about July 28, 1956 and the stars are now separating. Brief descriptions of the spectroscopic peculiarities that occur are given by WRIGHT and McKELLAR (1956) and by McKELLAR, WRIGHT, and FRANCIS (1957). These authors also list references to earlier work from low dispersion spectrograms. There seems little doubt that velocities as high as 50 km/s occur in the atmosphere of the M-type supergiant.

3'2.2. *Main-sequence stars.* — Macroscopic turbulence is observed in the extensive envelopes or shells surrounding a few early-type main-sequence stars. Most ordinary, dwarf or main-sequence stars are not surrounded by

observable shells, thus the data to be summarized now are characteristic only of a small fraction of the stellar population. Many main-sequence stars may have outer atmospheres similar to the solar chromosphere and corona, but at present there is no method of observing envelopes which contain little material.

The profile of  $H_{\alpha}$  is one of the most sensitive indicators of the velocity fields in the envelopes around early-type stars, for  $H_{\alpha}$  is an intrinsically strong spectral line from an abundant element. Even though the fraction of neutral hydrogen lies in the range  $10^{-3}$  to  $10^{-6}$  over much of a B-type shell, there are still enough atoms emitting and absorbing the Balmer lines to make the hydrogen lines sensitive probes of physical conditions in the shells.

When one is concerned with a low-density extensive atmosphere around a star, a spectral line will appear in emission if the shell is transparent to radiation from the neighbouring continuous spectrum, but opaque to line radiation, and if the shell is considerably bigger than the star. A few spectral lines are selectively excited in emission by special processes involving unobservable, strong emission lines in the extreme ultraviolet.

The available information about velocities in the extended atmospheres surrounding some main-sequence stars is summarized in Table IV. The various items are explained in the following paragraphs.

TABLE IV. *Velocities occurring in the extended atmospheres around main-sequence stars.*

Type of star	Radial velocity range (km/s)	Shape of absorption lines (km/s)	Shape of emission lines (km/s)	Expanding shell (km/s)	Chief objects studied
Be			200 ÷ 400		$\beta$ CMi, $\eta$ Tau, $\alpha$ Dra
B shell	10 to 80	50	600 ÷ 1000	?	48 Lib, $\zeta$ Tau
Of	30		100 ÷ 1500	?	9 Sge
WC	30		700 ÷ 1000	— 1200	H.D. 192103
WN	30		1000 ÷ 1500	— 1400	H.D. 192163

a) Be stars and B-type shell stars. When emission lines of hydrogen are observed superimposed on a B-type spectrum, the star is called a Be star. If in addition extra absorption lines (which usually mimic an A-type supergiant spectrum) are observed, the star is called a B-type shell star. A shell star appears to be similar to a Be star, the outer envelope, or shell, merely being larger and containing more material. These outer envelopes are frequently temporary features. Some stars have been observed to vary from B to Be, to shell star in less than 50 years. Many spectra have been taken of a few bright shell stars, and each series of observations usually

gives ample evidence that the material in the shell is moving with velocities of ten to several hundred kilometers per second. The motion is largely directed outwards. Gradients of velocity sometimes exist in a shell. This is indicated

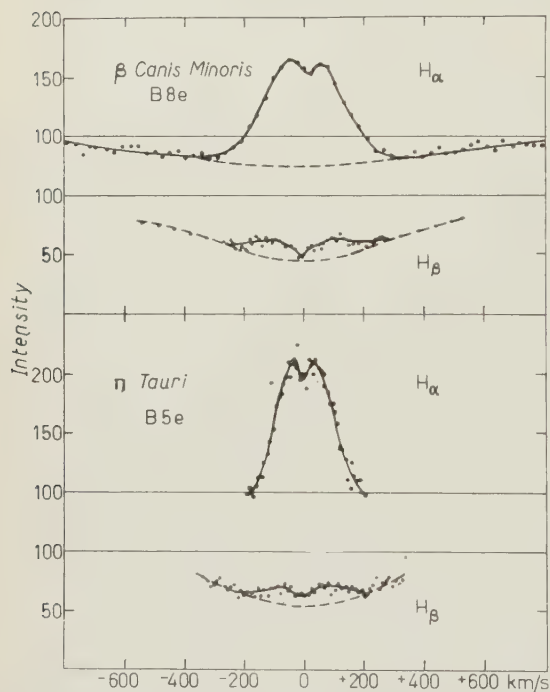


Fig. 6. — Profiles of  $H_{\alpha}$  and  $H_{\beta}$  in the spectra of the Be stars  $\beta$  *Canis Minoris* and  $\eta$  *Tauri*. In these stars, the width of the  $H_{\alpha}$  emission is about the same as that of the  $H_{\beta}$  emission on a velocity scale.

is about the same as that of the  $H_{\beta}$  emission profile. A few atoms appear to have velocities up to 400 km/s though most of the emission occurs within 200 km/s of the line centre. Figs. 8 and 9 present line profiles of  $H_{\alpha}$  and  $H_{\beta}$  in the spectra of the shell stars 48 *Librae* and  $\zeta$  *Tauri*. Two points are significant: (i) the emission is much wider and stronger in the shell stars than in the Be stars, and (ii) the extent of the wings in shell stars suggest velocities approaching 1000 km/s. It is not certain whether the extent of the emission line profile in shell spectra is due solely to the velocity field, or whether it is partially due to abundance broadening (radiation broadening wings). It is quite unlikely that Stark broadening is significant at the estimated density of shells,  $10^{11}$  particles/cm<sup>3</sup>. Significant changes in the velocity fields in shells usually occur slowly. Very rapid changes, in periods of the

by the observation that sometimes the absorption lines are asymmetrical, and by Merrill's discovery that in some stars the velocity shown by a high member of the Balmer series, say  $H_{25}$ , is more negative than that shown by an early member of the series, say  $H_{\gamma}$ . Usually the « Balmer progression », as this phenomenon of changing velocity with series member is called, is such to indicate that the inner layers of the shell are moving outwards more rapidly than the outer layers. MERRILL has observed one or two reversed Balmer progressions, but in these cases the velocity spread is never so large as when the motion is predominantly outwards.

Profiles of  $H_{\alpha}$  and  $H_{\beta}$  in the Be stars  $\beta$  *Canis Minoris*,  $\eta$  *Tauri* and  $\kappa$  *Draconis* are shown in Figs. 6 and 7. The width of the  $H_{\alpha}$  emission profile

order of a day, are not common. Some shells appear to be stationary, undergoing no change for years; others vary in a quasi-periodic manner. For a summary of further details of shell spectra, see UNDERHILL (1959a). The underlying B-type star is rotating rapidly in all cases which show strong shell spectra.

The radial velocity fluctuations of shell stars are usually in the range 10 to 20 km/s but outward directed velocities of 70 to 80 km/s are observed at times in the shell of 48 *Librae*, to give one example.

The shapes, strengths and displacements of the relatively sharp absorption shell lines reflect the changing density and velocity fields of that part of the shell which is projected against the disk of the star. However, since the emission lines arise from the whole of the optically thin atmosphere, the profiles of the emission lines reflect the overall velocity field of the shell.

It is interesting that the few quantitative observations that are available indicate that the wings

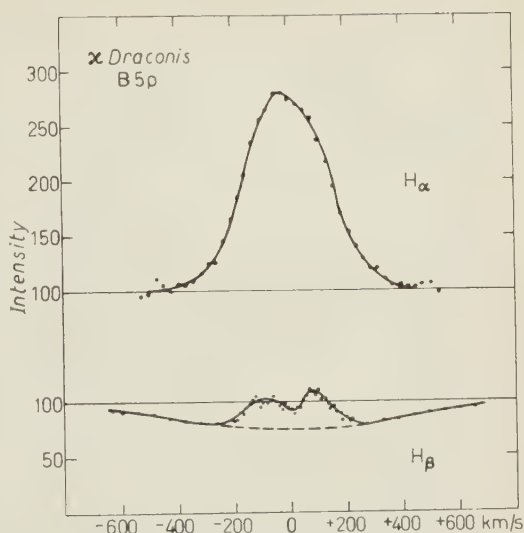


Fig. 7. - Profiles of  $H_{\alpha}$  and  $H_{\beta}$  in the spectrum of the Be star  $\alpha$  *Draconis*. Here the  $H_{\alpha}$  emission is more intense and it is wider than the  $H_{\beta}$  emission.

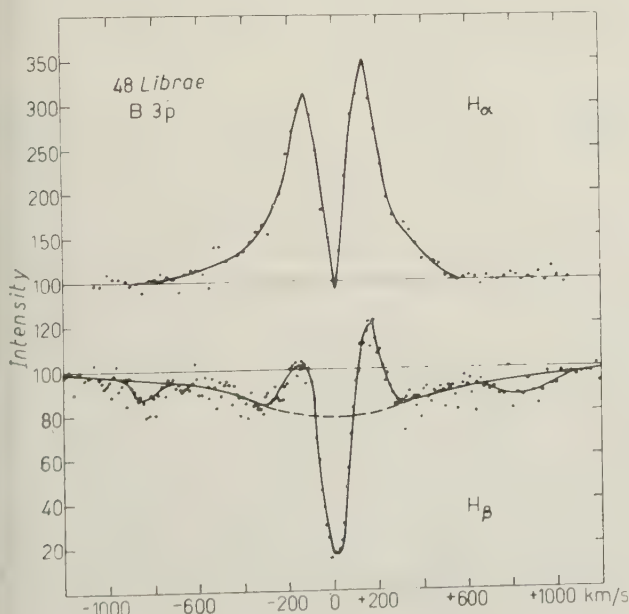


Fig. 8. - Profiles of  $H_{\alpha}$  and  $H_{\beta}$  in the spectrum of the shell star 48 *Librae*. Here the emission at  $H_{\alpha}$  is much stronger and wider than at  $H_{\beta}$ , and there is a strong self-reversal in each line due to the relatively dense material lying in front of the stellar disk.



of the emission  $H_\alpha$  profile in *48 Librae* remain constant in shape, even though the local velocity fields revealed by the absorption components change by a large amount in a few years. An expanding, low density atmosphere with local velocity irregularities in the inner more dense sections would explain the observations qualitatively.

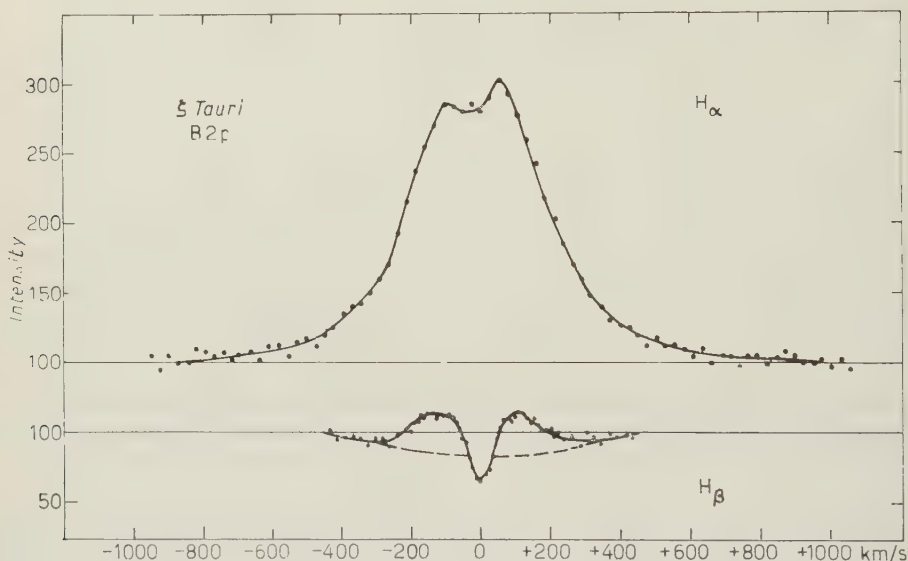


Fig. 9. — Profiles of  $H_\alpha$  and  $H_\beta$  in the spectrum of the shell star  $\zeta$  Tauri. Here the emission feature at  $H_\alpha$  has very wide wings and the central reversals are not so strong as for *48 Librae*.

b) Of stars. The Of stars are stars of type O8 and earlier in which the lines He II  $\lambda 4686$ , and N III  $\lambda 4634-41$  appear in emission. These lines may also appear in emission in the spectra of the O9 supergiants. (They probably are excited by selective processes in any low density gas around an object which has an effective temperature greater than about  $30\,000^\circ$ ). Little quantitative work has been done on Of spectra, for there are few bright Of stars accessible from the northern hemisphere. It would appear that the Of stars are related to the Be and B-type shell stars. Each Of star probably consists of a main-sequence O-type star surrounded by a more or less extensive envelope or shell.

R. WILSON (1957) from detailed study of low dispersion spectra obtained at the Edinburgh Observatory made the significant discovery that the emission lines in Of stars have wide, low intensity wings extending some  $1\,500$  km/s or so from the line centre. Thus large motions occur in the envelopes around O-type stars. The more intense parts of the emission lines are relatively narrow, corresponding in the case of *9 Sagittae*, O7f, to a velocity spread of 100 to

500 km/s (UNDERHILL, 1959c). The radial velocity of 9 *Sagittae* varies irregularly with a range of 30 km/s (UNDERHILL, 1958).

c) Wolf-Rayet stars. The spectra of Wolf-Rayet stars are quite extraordinary in comparison to normal stellar spectra, for they consist of many wide, strong emission lines (frequently called bands though they have nothing to do with molecular band spectra), and a few comparatively sharp absorption components. The level of excitation is very high, the spectra consisting of lines of He I, He II, C III, C IV, N II to N V, O II to O VI, etc. A peculiar selectivity is apparent, in that when lines of C II, C III, and the spectra from the oxygen ions dominate, lines from the spectra of nitrogen ions are weak and *vice-versa*. The situation regarding interpretation is further confused by the fact that about half of the known Wolf-Rayet stars are binaries. One then obtains the spectra of both stars superposed. The companion is usually an absorption-line O or B-type star. Since the Wolf-Rayet stars accessible from the northern hemisphere are faint for observation at high dispersion, little work has been done with a spectral purity sufficient to resolve these complex spectra properly.

The following facts relevant to the present discussion were confirmed by a detailed study of the spectra of H.D. 192103, WC7 and of H.D. 192163, WN6, (UNDERHILL, 1959b). These two stars appear to be single. The spectrum of the WN star contains lines of a higher level of excitation than does the spectrum of the WC star. It was early recognized, from comparison with nova spectra, that the shapes of the emission lines in Wolf-Rayet spectra were probably determined by the velocity fields in the atmospheres of these stars, but a consistent picture was not developed, partly because the interpretation of certain key observations was not clear when the first work was done. In the light of present knowledge about the production of stellar spectra the following statements probably correctly describe the main features of the velocity fields in the atmospheres of Wolf-Rayet stars.

1. The velocity fields in the inner, more dense parts of the atmosphere are randomly directed. In H.D. 192103 (a Wolf-Rayet star with a spectrum of comparatively low excitation), the total half-width of the He II emission lines from the 3-*n*, 4-*n* and 5-*n* series is 1300 km/s; in H.D. 192163 (a Wolf-Rayet star with a spectrum of relatively high excitation), the total half-width is 2000 km/s. There is some evidence that self-absorption occurs for the strongest He II lines. Relatively sharp, displaced absorption components are not observed for the He II lines, thus the simple picture of emission and absorption in an expanding shell of moderate dimensions is not adequate to describe the situation. A similar conclusion may be drawn from the detailed study of the spectrum of *V444 Cygni*, an eclipsing binary, one component of which is a Wolf-Rayet star.

2. An expanding shell of low density gas does exist around Wolf-Rayet stars. This shell is at some distance from the stellar surface, for it is observable only in spectral lines which are enhanced in a low density gas which is irradiated by a high-temperature, dilute radiation field. Such lines are the He I lines arising from the  $2^3S$  metastable level and from the  $2^3P$  level, and equivalent lines in the spectra of C III and N IV which have electronic structures outside the closed  $1s^2$  shell similar to that of He I. These lines appear in absorption, and they are the only obvious absorption lines appearing in the spectra of H.D. 192163 and H.D. 192103. The *outflow* motions made visible in this way are  $-1200$  km/s for H.D. 192103 and  $-1400$  km/s for H.D. 192163. In the cooler star, H.D. 192103, the C III line  $\lambda 5696$  appears in emission. The profile of this line is wider and more flat-topped than the profiles of the He II lines. The C III  $\lambda 5696$  line may be selectively excited in a low density gas which contains  $C^+$  ions and through which a flux of He II  $\lambda 303$  is flowing (UNDERHILL, 1957). The total half-width of  $\lambda 5696$  in H.D. 192103 is  $1860$  km/s. Thus it seems reasonable to conclude that the profile of  $\lambda 5696$  confirms the existence of the low density, expanding shell observed by means of the absorption lines. This expanding envelope is in addition to the denser atmosphere in which most of the spectral features are formed.

So little is understood about the excitation and production of Wolf-Rayet spectra at the present time that no secure guess may be made about the temperatures of these objects. It would appear that the radiation field corresponds to temperature in the range  $40\,000^\circ$  to  $100\,000^\circ$ . Qualitatively there is a considerable resemblance between the atmospheres and envelopes of Be stars (including shell stars), Of stars and Wolf-Rayet stars and the motions therein.

d) Binary stars: In many close binary systems, particularly those containing early-type stars, there is evidence of streams of gas enveloping one or both stars. A considerable body of literature exists on this subject. Such streams of gas may be related to the evolutionary progress of stars and to problems of mass loss. A brief, qualitative summary of some of this work has been given by STRUVE (1958). Further observations of the changing velocity field associated with some peculiar systems are summarized by MERRILL (1958). The velocity fields and density of gas shown by these objects are not unlike the velocity fields and density occurring in the extensive atmospheres surrounding supergiants and some main-sequence stars.

#### 4. - Summary of results and interpretation.

(i) The largest velocities are observed in the atmospheres surrounding the hottest objects. Thus the widest spectral lines reported here are for the WN6 object H.D. 192163, the average velocity of the atoms being in the

neighbourhood of 1 000 km/s. This star also exhibits the largest outflow velocity that was observed, namely — 1400 km/s. The radial velocity fluctuations have a larger range on the average, *viz.* 30 km/s in the early-type supergiants than in the later-type supergiants where the range is 4 to 8 km/s.

(ii) It appears that given two objects of approximately the same temperature (as determined from the radiation field), larger turbulent velocities are observed for the object with the more extensive atmosphere. Thus atmospheric velocities are greater in the atmospheres of supergiants than in the atmospheres of giant stars or of dwarf stars. Still larger velocities are observed in the shells around some early-type main-sequence stars than in the less extensive atmospheres of the early-type supergiants.

(iii) There is ample evidence that the velocity fields of stellar atmospheres are irregular. The velocity distribution changes from place to place in the stellar atmosphere, witness the multiple chromospheric Ca II *K* lines in *31 Cygni* and in *VV Cephei*. It also changes from time to time, witness the changes of shape of lines in the spectra of extremely luminous stars such as *ε Aurigae*, and the irregular variations of the radial velocity of supergiants.

(iv) Much of the material in extended atmospheres appears to be moving in random directions. However, on the whole there is also evidence that a low density shell of gas is streaming from most stars. The expanding circumstellar shells in the late-type supergiants appear to be moving outwards with respect to the stellar surface at speeds of 10 to 20 km/s. In the Wolf-Rayet stars the most probable outward velocity is greater than 1 000 km/s. The symmetrical wings of the  $H_{\alpha}$  emission profile in Be and shell stars may be explained by a shell expanding with a velocity of several hundred km/s in addition to the turbulent velocity field made visible by the absorption lines. The changing  $H_{\alpha}$  emission profiles of the B-type supergiants, and in particular the violet displacement of the absorption cores also indicate predominantly outward motion.

(v) In view of the simplifications in physical theory that are made in the theory of the curve of growth so as to obtain representations of the situation that may be handled by relatively simple mathematical methods, it is difficult to decide exactly what the so-called microscopic turbulent velocities mean. Certainly they can represent only a small part of the velocity field that exists, for the Doppler shifts due to large velocities effectively change the wave-length of the spectral line sufficiently that so far as the curve of growth is concerned the atoms with large velocities do not contribute to the absorption line at all. The observable quantities used to obtain information about large scale motions in stellar atmospheres, on the whole, reflect prop-



erties of the atmosphere at high levels, far from the stellar photosphere. The equivalent widths used to form curves of growth, on the other hand, reflect the properties of intermediate and deep layers of the atmosphere. Thus the results summarized as «microscopic turbulence» refer to a different part of the atmosphere, on the average, than do those summarized as «macroscopic turbulence».

(vi) It has several times been noted that if one observes only the profiles of spectral lines of giants and supergiants in the blue-violet region at a dispersion of about  $10 \text{ \AA/mm}$ , one cannot decide between rotation as the dominant means of broadening and a symmetrical macroturbulence. SLETTEBAK (1956) and ABT (1958), in particular, have favoured the hypothesis of slow rotation. In fact ABT has attempted to work back from the magnitude of this «rotation» to find the point on the main sequence from which the stars now observed as supergiants may have originated. I see no reason compelling one to accept this line of thought. Rather I believe that the observed broadened profiles of the supergiants are due chiefly to large scale, randomly directed motions of the gases in the outer layers of these stars. The principle facts supporting this conclusion are (1) the randomly occurring changes of line shape which are observed for the most luminous stars when their spectra are observed at very high dispersion, and (2) the irregular radial velocity fluctuations found for all supergiants that have been observed in detail. HUANG and STRUVE (1954) also came to the same conclusion on statistical grounds.

It is clear that most, if not all, early-type main-sequence stars rotate rapidly. It is far from clear whether the less numerous early-type stars having luminosities distinctly brighter than the main sequence (that is, the Ib and Ia supergiants) rotate at all. It is certain that large scale motions exist in the atmospheres of these stars.

#### REFERENCES

- ABT, H. A. 1957, *Ap. J.*, **126**, 158.  
 ABT, H. A. 1958, *Ap. J.*, **127**, 658.  
 ALLER, L. H. 1956, *Ap. J.*, **123**, 133.  
 BEALS, C. S. 1951, *P. Dom. Ap. O.*, **9**, 83.  
 BIDELMAN, W. P. and McKELLAR, A. 1957, *P.A.S.P.*, **69**, 31.  
 BÖHM-VITENSE, E. 1956, *P.A.S.P.*, **68**, 57.  
 BONSAK, W. K. and GREENSTEIN, J. L. 1960, *Ap. J.*, **131**, 83.  
 BURBIDGE, E. M. and BURBIDGE, G. R. 1955, *Ap. J.*, **122**, 396.  
 BURBIDGE, E. M. and BURBIDGE, G. R. 1956, *Ap. J.*, **124**, 116.

- BUSCOMBE, W. 1951, *Ap. J.*, **114**, 73.
- CAMPBELL, W. W. and MOORE, J. H. 1928, *P. Lick O.*, **17**, 105.
- GOLDBERG, L. 1939, *Ap. J.*, **89**, 623.
- HUANG, S. S. 1950, *Ap. J.*, **112**, 399.
- HUANG, S. S. 1952, *Ap. J.*, **115**, 529.
- HUANG, S. S. and STRUVE, O. 1952, *Ap. J.*, **116**, 410.
- HUANG, S. S. and STRUVE, O. 1953, *Ap. J.*, **118**, 463.
- HUANG, S. S. and STRUVE, O. 1954, *Ann. d'Ap.*, **17**, 85.
- HUANG, S. S. and STRUVE, O. 1955, *Ap. J.*, **121**, 84.
- KEENAN, P. C. and MORGAN, W. W. 1951, *Astrophysics* (Ed.: HYNK) (New York) p. 12.
- McKELLAR, A., WRIGHT, K. O. and FRANCIS, J. D. 1957, *P.A.S.P.*, **69**, 442; *Contr. Dom. Ap. O.*, No. 58.
- McKELLAR, A. and PETRIE, R. M. 1959, *P. Dom. Ap. O.*, **11**, 1.
- McKELLAR, A., ALLER, L. H., ODGERS, G. J. and RICHARDSON, E. H. 1959, *P. Dom. Ap. O.*, **11**, 35.
- MERRILL, P. W. 1958, *Etoiles à Raies d'Emission*, Université de Liège, p. 436.
- MICZAIKA, G. R. and WADE, M. J. 1958, *Ap. J.*, **127**, 143.
- MOORE, J. H. 1932, *P. Lick O.*, **18**, 211.
- PADDOCK, G. F. 1935, *Lick O. B.*, **17**, 99.
- SLETTEBAK, A. 1956, *Ap. J.*, **124**, 173.
- STRUVE, O. 1946, *Ap. J.*, **104**, 138.
- STRUVE, O. 1958, *Etoiles à Raies d'Emission*, Université de Liège, p. 377.
- STRUVE, O. and ELVEY, C. T. 1934, *Ap. J.*, **79**, 409.
- UNDERHILL, A. B. 1957, *Ap. J.*, **126**, 28; *Contr. Dom. Ap. O.*, No. 55.
- UNDERHILL, A. B. 1958, *P. Dom. Ap. O.*, **11**, 143.
- UNDERHILL, A. B. 1959a, *J.R.A.S. Can.*, **53**, 72.
- UNDERHILL, A. B. 1959b, *P. Dom. Ap. O.*, **11**, 209.
- UNDERHILL, A. B. 1959c, *P. Dom. Ap. O.*, **11**, No. 15.
- UNSÖLD, A. and STRUVE, O. 1949, *Ap. J.*, **110**, 455.
- VAN DE HULST, H. C. and REESINCK, J. J. M. 1947, *Ap. J.*, **106**, 121.
- VOIGT, H. H. 1952, *Z. Ap.*, **31**, 48.
- WEHLAU, W. H. 1956, *Ap. J.*, **123**, 34.
- WILSON, O. C. and BAPPU, M. K. V. 1956, *Ap. J.*, **125**, 661.
- WILSON, R. E. 1953, *Catalogue of Radial Velocities*, Carnegie Inst. Washington, p. 601.
- WILSON, R. 1957, *P. Roy. O. Edinburgh*, **2**, 61.
- WORLEY, C. E. 1956, *P.A.S.P.*, **68**, 62.
- WRIGHT, K. O. 1947, *J.R.A.S. Can.*, **41**, 49.
- WRIGHT, K. O. 1955a, *Trans. I.A.U.*, **9**, 742.
- WRIGHT, K. O. 1955b, *J.R.A.S. Can.*, **49**, 221; *Contr. Dom. Ap. O.*, No. 46.
- WRIGHT, K. O. 1959, *P. Dom. Ap. O.*, **11**, 77.
- WRIGHT, K. O. and McKELLAR, A. 1956, *P.A.S.P.*, **68**, 465; *Contr. Dom. Ap. O.*, No. 49.
- WRIGHT, K. O. and LEE, E. K. 1959, *P. Dom. Ap. O.*, **11**, 59.

## PART II.

### General Summary of Results on « Astronomical Turbulence » in Stellar Atmospheres.

#### Discussion.

Chairman: A. UNSÖLD

(*Ed. Note:* The discussion opened, as it did in Part I, with questions by aerodynamicists on the astronomical jargon. A first part, devoted to clarification of the meaning of the stellar classification scheme in its implications on the physical characteristics of the stars discussed, is condensed and summarized. A second part began as a request for a re-explanation of the curve of growth, and continued as a question on the reliability of this technique. The explanation has been suppressed as duplicating material in Part I; the questions on the reliability of the technique have been retained as the beginning of the discussion proper. Miss UNDERHILL has also added an explanatory section in her text, immediately adjacent to Table I, to clarify some of these points.)

*Summary of physical implication of classification scheme, based on remarks by A. UNDERHILL, A. J. DEUTSCH, E. SCHATZMAN, A. UNSÖLD.*

The spectral class of a star is specified by a letter; its luminosity class, by a roman numeral. The spectral class was originally a wholly empirical assignment, based on an empirical regular progression in behavior of the features of the spectrum of the star, pre-dating the development of theoretical understanding of atomic spectra. The Saha-Fowler introduction of thermodynamic-equilibrium statistical mechanics to describe the ionization and excitation state of the gas in the stellar atmosphere, treated as an isothermal region, showed that the spectral sequence could be interpreted in terms of a monotonic decrease of this atmospheric temperature from the « bluest » stars (type O) at one end of the sequence to the « reddest » (types R, N, S), at the other. Moreover, again applying thermodynamic equilibrium relations, roughly this same temperature value gave a good description of the distribution of energy in the continuous spectrum, and the total emission per unit surface area. A rough theory of radiative transfer through the atmospheric regions gives a reasonable

quantitative representation of the direction of difference in temperature values—for given spectral class—needed to represent spectrum, distribution of energy in the continuum, and surface-flux of radiation. Excitation and ionization temperatures,  $T_{ex}$  and  $T_{ion}$ , refer to the line spectrum; color temperature,  $T_c$ , to the distribution of energy in the continuum; effective temperature  $T_{eff}$ , to the surface-flux of radiation. Roughly, temperatures vary from  $30\,000^\circ$  for an O star to  $2\,000^\circ$  for a K star, in the normal atmospheric regions. (This takes no account of chromospheric or coronal phenomena.) Any star with a temperature above about  $10\,000^\circ$  is called «early-type»; anything cooler is called «late-type». (For detailed discussion of all these points, cf. A. UNSÖLD: *Physik der Sternatmosphären*.)

The Russell-Herzsprung diagram was originally a wholly empirical discovery showing that total luminosity of the star and spectral class were not wholly independent, nor were they single-valuedly related. Three broad luminosity groups were originally found for a given spectral class: supergiants, giants, and dwarf or main-sequence stars—in order of decreasing luminosity. Only later was it found that these terms also refer to stellar dimensions, and that the pressure in the atmospheric regions varies oppositely to the luminosity, giving rise to measurable spectral differences, which permit luminosity classes to be established from spectral measures alone. The classes are now sharper than the supergiant, giant, dwarf categories: roughly, Ia and Ib refer to the first; II and III, to the second; IV and V, to the third. Pressures in supergiant atmospheres are roughly 100 times less than in dwarf atmospheres.

Current theories of stellar evolution regard the mass of the star as the basic physical parameter varying along the main sequence. A star condenses out of the interstellar medium, taking very quickly a place on the main sequence determined uniquely by its mass. The star remains on the main sequence so long as it generates energy wholly by thermonuclear processes, then moves off to the right of the main sequence into the giant or supergiant region depending upon its mass, and ultimately comes back to the left and falls into the white dwarf category. The essential point here is that the place the star occupies on the diagram is a unique function of its mass and the degree of exhaustion of its thermonuclear resources. (For further reference, cf. M. SCHWARZSCHILD: *Stellar Evolution*.)

— E. N. PARKER:

A question on the curve of growth. There are gradients of temperature, and various lines may come from different heights in the atmosphere. To what extent is the curve-of-growth analysis uncertain due to temperature gradients, and the fact that various parts of individual lines would come from various levels. What I am driving at is the physical significance of micro-turbulence. I am not yet convinced it exists, and I want to hear some arguments on this



point. Can it be shown by formal calculation that we must believe in micro-turbulence as turbulence, or should we call it a discrepancy factor to be settled in the future?

— M. MINNAERT:

Actually, for each line the curve of growth should be different, and should correspond to slightly different atmospheric layers. The flat section will come slightly lower or higher, and this might confuse us and modify somewhat the microturbulence found. But these are refinements which do not remove the well-established main effect of microturbulence found by everybody.

— A. UNDERHILL:

I think the uncertainty due to the fact that really the physical quantities vary through the atmosphere, will introduce something of the order of magnitude of 0.1 in the log as a probable error in the velocity. One could of course refer to what we call microturbulence as a discrepancy factor. The factor was originally given the name «turbulence» because it was recognized that a random velocity, entering the curve-of-growth structure in the same manner as the thermal velocity, would act in exactly the correct way to remove the discrepancies from results based on thermal velocities alone.

— A. UNSÖLD:

We should follow Parker's question concerning the significance and accuracy of the micro-turbulent velocities  $\xi_{\text{turb}}$  determined from curves of growth somewhat further. The thermal velocities  $\xi_{\text{therm}}$  of heavier elements like Ti, Fe... in stars of medium temperature like the sun ( $\sim 6\,000^\circ\text{K}$ ) are of the order of 2 km/s. If micro-turbulent velocities are added, the Doppler width  $\Delta\lambda_{\text{D}}$  of a line increases in the ratio  $\sqrt{(\xi_{\text{therm}}^2 + \xi_{\text{turb}}^2)}/\xi_{\text{therm}}$  and the almost horizontal part of the curve of growth moves upward in the same way. The question is, how accurate can its location be determined from the theory of stellar atmospheres in case of no turbulence? The answer is that the intrinsic uncertainties of the model atmosphere plus the errors of (reasonably good) measurements produce an inaccuracy in the height of the flat part of the curve of growth of about  $\pm 30$  percent. That means: Turbulent velocities of the order of 5 to 2 km/s or larger can be determined quite well and are certainly real. It should be noticed further that the observed micro-turbulent velocities are subsonic, relative to the velocity of sound in hydrogen, which is the most abundant element.

Next, I don't understand why PARKER emphasizes so much the temperature gradients. In computations based on a model atmosphere, the temperature gradients in the atmosphere are taken into account.

— E. N. PARKER:

I find in the literature wide variations in estimate of temperature and temperature gradient in the region of line-formation. What does one use?

— A. UNSÖLD:

Well, there is the question of how accurate are the calculations of model atmospheres. Up to about 1946, we used so-called «grey models», based on the assumption of no frequency-variation of continuous absorption coefficient, and we know now that this simple assumption must be amended. The temperature actually decreases toward the surface faster than assumed earlier. However, the calculation of a really good model atmosphere is a lengthy job, and has been done so far only for a few stars.

— A. UNDERHILL:

Everyone is worried about details; and as UNSÖLD has emphasized, to obtain detailed answers requires much detailed analysis. The whole of the data I have presented has been obtained by straightforward and simple methods of analysis. A few detailed cases which UNSÖLD has worked out, and tried to improve by taking into account these physical details more correctly, has confirmed that these numbers give the proper order of magnitude. But please don't think that these numbers have all been ascertained by as detailed methods as he has mentioned; they have mostly been obtained by quite crude analysis.

— M. J. SEATON:

If one considers micro-turbulence inferred from curve of growth to be a discrepancy-factor, then at least one always gets positive turbulence velocities. If one ascribed the factor to non-LTE effects, would he expect the discrepancy to be always of the same sign?

— R. N. THOMAS:

Yes. Non-LTE effects make the line deeper.

— R. B. LEIGHTON:

On the question of spectral lines coming from different levels, it is not clear to all the aerodynamicists why lines coming from a higher state of excitation tend to be formed lower in the atmosphere than those from states of lower excitation.

— A. UNSÖLD:

The temperature increases as one goes deeper into the atmosphere—we do not consider here the chromosphere. Thus, excitation and ionization follow

the Boltzmann and Saha equations, and excitation increases downward. (*Ed. Note:* Relative to the chromospheric influence on position of line-formation, cf. ZIRKER: *Ap. J.*, **127**, 680 (1958)).

— W. B. THOMPSON:

Come back to Parker's original question. Could we be told exactly what physical assumptions underlie the curve-of-growth analysis, and what sort of calculations have been carried out? It seems to me that the hydrodynamic problem posed in finding these extremely high, very fine scale velocities is really a very severe one, and we should like to understand just how sure you are that these results are actually meaningful.

— A. UNDERHILL:

You put two types of questions forward. Let me answer the second: why are we sure that great velocities and peculiar velocities exist? The quantity called micro-turbulence is based on an analysis using only equivalent widths—the integrated line-profile. The line-shape does not enter. In my opinion, too much detail cannot be gained from this quantity; one can get only an order of magnitude of what may be called a discrepancy factor. The only way you can find detail is when you turn to macro-turbulence, the analysis of line-profiles. Here the sun comes into its own; stars cannot be studied in all desired detail.

— A. J. DEUTSCH:

I would present evidence for the reality of at least the larger values of micro-turbulence listed by Miss UNDERHILL. When the micro-turbulence gets larger than  $(2 \div 3)$  km/s, as it commonly does in giant stars, we then see it affecting the lines in two ways: it changes their equivalent widths and it also changes their profiles. The profile broadens by an amount which I think can be shown on rather simple grounds to be inadmissibly large to be accounted for in terms of temperature gradients in the stellar atmosphere. For example, in stars where we know that the atmospheric temperatures in the relevant layers are of the order of  $5000^\circ\text{K}$ , and the thermal velocities of the order of 2 km/s, nevertheless the line has an overall width of 5 or 6 km/s, while still not showing damping wings. I think the only way we can understand this is to suppose that, in addition to the thermal motion, there is another kind of motion; this has been called turbulence. I would like to concede that one does not have this kind of evidence for the sun or for most stars like the sun. But, as we pass to the stars where what we called the turbulence velocity becomes higher, we get a transition region where simultaneously we see the effect of the raising of the horizontal part of the curve of growth, and the widening of the line-profile, which I believe cannot be interpreted in terms of tempera-

ture gradients. Of course, when we go to the very extreme case of the supergiants, this becomes exceedingly obvious. There, however, the velocity has become sufficiently large that the principal effect lies in the broadening of the profile and we no longer have the principal effect in the equivalent widths. I think it is partly for this reason—that when we go to the cases of extreme turbulence we get this additional evidence from line profiles—that many astronomers have been reluctant to abandon the idea that there is a similar phenomenon at work here on the sun and in stars like the sun.

— A. UNSÖLD:

What you show, is that in every case where one has pronounced macro-turbulence, one has also—as he would expect—micro-turbulence, of somewhat smaller size. There remains the question of how do we establish the values of these turbulent velocities.

— M. KROOK:

I am correct in saying that the way one makes these calculations is to assume that there are no motions other than thermal, and neglects the lines, then computes the thermal structure of the atmosphere? Then using this thermal structure, you compute what the lines would look like, again in an atmosphere with only thermal motions? If you find a discrepancy, you assign it to turbulence? In other words, one does not calculate the formation of a line in an atmosphere in which turbulence is actually present, and may affect both thermal structure and line absorption coefficient?

— A. UNSÖLD:

Analysing a stellar spectrum is like solving a cross-word puzzle. You have quite a number of constants to determine from a great many observational data, and you begin from some starting approach and proceed until something doesn't check for consistency. Then you start again. It is difficult to explain the whole procedure quickly. The detailed analysis of one stellar spectrum by an experienced man takes about 2 years.

— W. B. THOMSON:

Can you calculate the atmospheric structure, taking into account the convection and turbulence?

— A. UNSÖLD:

In general, one does this. The influence of the turbulent velocities on the stratification of the atmosphere through the dynamical equations is rather small. The essential point in the analysis of a spectrum is first to get a reliable



value of the temperature structure, because temperature enters in a very sensitive way into all the subsequent calculations. Subsonic micro-turbulence produces only minor correction on the temperature structure.

— E. N. PARKER:

Where can I find how this correction to the thermal structure is calculated? This is of interest to ~~the~~ aerodynamicist because this is the aerodynamic part of the determination of structure.

— A. UNSÖLD:

This point is unimportant for what Miss UNDERHILL has been talking about.

— H. PETSCHKE:

We are asking not only about convection due to turbulence, but also about the energy dissipation due to large-amplitude velocity. Can you prove what you said, simply?

— R. N. THOMAS:

The argument is that micro-turbulent velocities are about 2 km/s; thermal velocity, 10 km/s; thus Mach number, about  $\frac{1}{5}$ , so energy dissipation from micro-turbulence is small. The question remains about the implication of the macro-turbulent velocities quoted by Miss UNDERHILL, where the Mach number considerably exceeds one. The temptation among astronomers is to say: if we have to correct something, it lies in this latter aspect. This is a summary of a viewpoint, not a defense of it.

— A. UNSÖLD:

Agreed on the micro-turbulence. In the chromosphere and corona, where the heat dissipation is a vital point, there are of course a different set of problems, but these will be discussed later.

— W. B. THOMPSON:

If micro-turbulent velocities were observed to be supersonic, I think it would be an extraordinary thing from the hydrodynamic viewpoint. If they are subsonic, it would be surprising if they did not actually exist. So let me ask for complete clarification on one point. In *micro*-turbulence, not *macro*-turbulence, do you ever see extreme supersonic velocities?

— A. UNSÖLD:

From Miss Underhill's results, *micro*-turbulent velocities are of the order  $(2 \div 5)$  km/s, which is the same size as, or larger than, the thermal velocities

for the atoms observed, which are the heavier elements. But the sound velocity in the atmosphere is practically that for hydrogen, some  $(8 \div 10)$  km/s. Therefore the observed micro-turbulent velocities are generally subsonic, and their dynamical pressure as well as their energy dissipation are of secondary importance.

(*Ed. Note:* The significance of such a generalization on the empirical values for micro-turbulence from curve-of-growth studies cannot be overemphasized. Miss UNDERHILL gives values up to 20 km/s for stars in luminosity class Ia; the summary by K. O. WRIGHT, *Trans. Int. Astr. Union*, **9**, 739 (1955) gives variety of cases exceeding 10 km/s for a range of spectral classes. One asks the significance of such results, relative to Thompson's question, to the applicability of the curve-of-growth methodology, and to the accuracy of the empirical results.)

— G. ELSTE:

Consider a simple picture showing up micro- and macro-turbulence in an atmosphere with a single velocity field. There may be a large outward motion in deep layers which becomes smaller and smaller with increasing height. And at another point of the atmosphere there may be inward motion with a certain velocity gradient. Consider a line being effectively formed in a certain layer of finite thickness. Take the average velocity over this layer in both the upward and downward moving region. The difference of these average velocities will smear out the spectral line without changing its total absorption. This we call macro-turbulence. The scattering of the individual radial velocities within the layer around the average velocity in each region acts on the line like an additional kinetic temperature and changes its width as well as its total absorption. This effect we call micro-turbulence. For another line, having different excitation conditions, the position and thickness of the absorbing layer may be different, resulting in different behavior of the line.

— M. MINNAERT:

ELSTE has given a very precise account of an observational situation. These effects are observed at least as well at the limb as at the center of the solar disk. This shows that there are tangential as well as radial currents; apparently there is a field of large and small scale random velocities, which one should be inclined to connect with the occurrence of vortices, and which astrophysicists usually call turbulence.

— A. J. DEUTSCH:

It is my understanding that, as astronomers employ the terms, convection and micro-turbulence are not the same thing. I think that those of us who are persuaded of the existence, at least in some stars, of micro-turbulence are

by no means persuaded that this is a convective kind of circulation. Isn't it true that the relevant layers in the solar atmosphere are in radiative equilibrium, so one does not feel that here it is convection? Thus, one should insist that he do not designate as convection all kinds of turbulence in stellar atmospheres.

— A. UNSÖLD:

This point will be covered in detail in Part IV-A, by Mrs. BÖHM-VITENSE, because the sun is the only star where these things have been studied in sufficient detail. At the present session, we do not yet consider the mechanism producing the observed velocities.

— E. SCHATZMAN:

Let me give a quick picture of the different kinds of motions we postulate, where they occur, and their relation to the observations. In the lowest observed atmospheric regions, we have a convective zone; above that a radiative zone; above that a chromosphere. The convective zone is the seat of convective motions which lead to the production of compression waves, which propagate outward, and they decay in the upper part of the radiative zone or in the chromosphere. The motions of the convective zone are usually supposed to appear in the curve of growth. The motions in the upper regions are probably the source of the line-broadening. In the case of Wolf-Rayet stars, we do not know the origin of such large velocities as are observed. In stars with extended envelopes, we have to consider the effect of the Keplerian motions of the envelope around the star.

— E. SPIEGEL:

I want to draw attention to a possibly useful observational approach in the study of motions in stellar atmospheres. In the case of stars of spectral type near B0, there are convective instabilities near the surface due to the second ionization of He. It might be possible to detect the effects of the resulting motions on the spectra in the following manner: One might expect that lines formed principally in rising hot masses of gas are shifted to the violet while those formed in descending masses would show a corresponding red shift. The magnitude of the shift should be less than that given by the sound speed in such stellar atmospheres, about 20 km/s. On the theoretical side we know from the work of TRAVING on the star *10 Lacertae* that such motions could exist without disturbing the radiative equilibrium, and would ordinarily escape notice.

There are not many data which are available for such an investigation, but STRUVE has kindly provided some radial velocities for *10 Lacertae*. Fig. 1

shows a plot of radial velocity against mean optical depth of formation as calculated by TRAVING. Lines for given ions have been grouped together and the size of the point in the plot is proportional to the number of lines measured. The asterisk represents 14 HeI lines.

One sees that there is some indication that a correlation may exist in the suggested way. The highly discrepant point at  $\bar{\tau} = .22$  is due to 6 lines of OIII which lie mainly in the UV.

Clearly, the data are not yet adequate for any conclusions to be drawn, but I would like to ask the observers whether they feel that with sufficient data, such studies might possibly be made definitive.

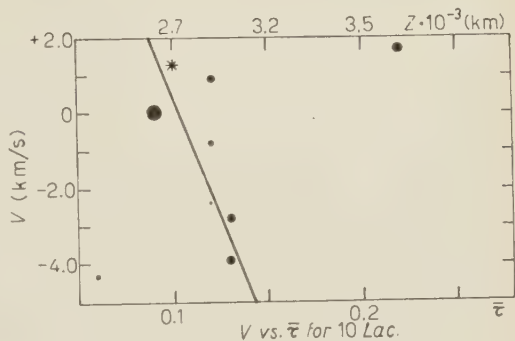


Fig. 1.

— A. UNDERHILL:

I question whether the size of the effect found is significant, relative to the uncertainties existing in the relative wave-length standards for the lines of HeI, CII, OII, OIII, and NIII used in the analysis. The velocity range found is 6 km/s, which corresponds to a  $0.08 \text{ \AA}$  shift in the  $4000 \text{ \AA}$  region where most of these lines lie. So the relative positions of the absolute wave-lengths of these lines must be known to higher accuracy than this  $0.08 \text{ \AA}$  in order that the differential measures be meaningful. In the preface to the Revised Multiplet Tables, Mrs. SITTERLY carefully remarks that the several elements are not necessarily on the same wave-length system, owing to the experimental difficulties associated with producing the lines. It is my impression that small differences exist, of the same size as the  $0.08 \text{ \AA}$  found here.

— E. N. PARKER:

Then do any radial velocities in the data presented mean anything?

— A. UNDERHILL:

Yes, because in measuring stellar radial velocities, the first thing you have to do is to set up a set of empirical wavelengths for each spectral type. In brief, you take spectra of sunlight reflected from the planets. You can compute the motion of a planet from its known position. Then you adopt a set of wavelengths that will reproduce the theoretical planetary velocities consistently within a fraction of one km/s. Then for each spectral class, and for



each spectral dispersion used in observing, you establish a mutually-consistent set of effective wave-lengths. This is particularly important for the question of blends in the spectral lines. Consider, *e.g.*, the HeI triplet lines. To obtain the correct radial velocity, do you adopt the wave-length of the strongest component, or do you take the mean of the three. The effective wave-length may change by something like  $0.1 \text{ \AA}$  depending upon your decision.

— E. SPIEGEL:

The second problem concerns the case of a binary star, *31 Cygni*, where one component, a K-star, has such an extended atmosphere that the ratio of the radius of the K-star to that of its B-star companion is of order  $10^2$ . The orbital plane lies roughly in the line of sight, so we observe the B-star passing behind the K atmosphere, acting as a probe, which enables us to study the conditions in the extended atmosphere. One observes absorption lines produced in the B-star spectrum by the K-star atmosphere. The present conclusions are based on an analysis by ALLER and myself, of measures by MACLAUGHLIN, made at the next-to-last eclipse. The data are incomplete because of cloudy nights, and not made at the highest resolution now available. We tried to take the autocorrelation of the mean velocity along the line of sight, as a function of radial position. Such an autocorrelation depends not only on position but also on time, since it takes the B-star several days to move the distance between points, which is some  $3 \cdot 10^6 \text{ km}$ . None the less, the autocorrelation function appears to be well-defined, dropping to zero at a distance of about  $2 \cdot 10^7 \text{ km}$ , agreeing well with the figures Miss UNDERHILL gave. These results are tentative, and push the available data to the limit.

— S. S. HUANG:

The so-called «turbulence» as used by stellar spectroscopists is not necessarily the turbulence as understood by aerodynamicists. Therefore it is unfortunate, if not misleading, for astrophysicists to use the name «turbulent velocities» to denote some parameters which are introduced to interpret stellar spectral lines. Then, what is the meaning of the so-called micro-turbulent and macro-turbulent velocities which have been discussed by UNDERHILL and which have caused quite long discussion in this symposium? In order to clarify this point, we have to consider the nature of stellar spectral lines because, after all, the turbulent velocities in stellar atmospheres as used by astrophysicists are derived entirely from spectral lines.

Consider a point  $(x, y)$  on the stellar disk. From the theory of radiative transfer which was discussed extensively by many speakers yesterday, we can derive, in principle at least, the line profile of the emergent light at the point,  $(x, y)$  as

$$(1) \quad I(\lambda, x, y, t; \bar{\alpha}, \bar{\beta}, \bar{\gamma}, \dots, a, \dots),$$

where  $t$  denotes the time of observation, while  $\alpha, \beta, \gamma, \dots$  denote a set of parameters depending on the layer of the atmosphere, *i.e.*

$$\alpha = \alpha(\tau), \quad \beta = \beta(\tau), \quad \text{etc.}$$

and  $a, \dots$  denote another set of parameters depending on the particular line concerned but independent of the depth  $\tau$ . In other words,  $a, b, \dots$  are atomic constants characteristic of the spectral lines, such as the transition probability while  $\alpha, \beta, \gamma, \dots$  may be temperature, pressure, magnetic field, etc. Thus,  $\alpha, \beta, \gamma, \dots$  enter into  $I(\lambda, \dots)$  through the source function in a complicated way. Expression (1) is only a rough approximation in saying that the effect of  $\alpha(\tau), \beta(\tau)$ , etc., on  $I(\lambda, \dots)$  can be represented by a single mean value  $\bar{\alpha}, \bar{\beta}, \dots$  respectively. What we actually observe is

$$(2) \quad F(\lambda, \alpha, \beta, \gamma, \dots, a, \dots) = \\ = \iiint I(\lambda', x, y, t, \alpha, \beta, \gamma, \dots, a, \dots) dx dy dt J(\lambda - \lambda') d\lambda',$$

where  $J(\lambda)$  is a normalized function known as the instrumental profile. The integral in (2) defines three kinds of broadening, *i.e.*

- 1) physical broadening which is due to  $\bar{\alpha}, \bar{\beta}, \bar{\gamma}, \dots, a, \dots$ ,
- 2) geometrical broadening which is due to the integration over  $x$  and  $y$ ,
- 3) operational broadening which is due to the integration over  $t$  and  $\lambda'$ .

With modern technology, we can reduce the exposure time to a very short interval and make the instrumental profile nearly a  $\delta$ -function. Then we can neglect the operational broadening altogether and reduce (2) to

$$(3) \quad F(\lambda, \bar{\alpha}, \bar{\beta}, \bar{\gamma}, \dots, a, \dots) = \iint I(\lambda', x, y, \bar{\alpha}, \bar{\beta}, \bar{\gamma}, \dots, a, \dots) dx dy.$$

A transfer theory, of which the curve-of-growth analysis plays only a part, should explain the profiles and consequently the equivalent widths, of all lines—from very weak to very strong—by assigning suitable values to  $\bar{\alpha}, \bar{\beta}, \bar{\gamma}, \dots$  in the atmosphere, while  $a, \dots$  are in principle known quantities. The curve of growth is a method to determine  $\bar{\alpha}, \bar{\beta}, \bar{\gamma}, \dots$  for the atmosphere concerned from a compromise over all lines.

It is now clear that a velocity field due simply to thermal motion is not enough to explain the behavior of all lines. In other words, the determined velocity from the curve of growth is too large to be accounted for by thermal

motion. The name «turbulence» is introduced to describe the excess velocity, and it is generally known as the «micro-turbulent velocity» in contrast to the «macro-turbulent velocity» which will be discussed later. The micro-turbulent velocity thus defined may not be a turbulent velocity in the sense of aerodynamicists. Indeed, the profiles— $I(\lambda, \dots)$  in the case of the sun and  $F(\lambda, \dots)$  in the case of the stars—may be explained by other means than the introduction of a velocity parameter. This is what THOMAS and his associates are trying to do. There is no reason to object, *a priori*, to Thomas' approach.

Next we consider the broadening due to the integrations over  $x$  and  $y$  i.e. the geometric broadening. Because of the up and down motions at different points on the stellar disk, the integration over  $x$  and  $y$  introduces further broadening, which is said to be caused by macro-turbulent motion. Astrophysicists usually assign one single velocity to denote the magnitude of the macro-turbulent motion. It is obvious that one single parameter is not enough to describe the motions over the stellar surface. For example, axial rotation of the star behaves exactly like macro-turbulent motion so defined. A single parameter cannot show such a difference. Here a function instead of a parameter has to be introduced to define the mode of motion on the stellar surface. This function is called the broadening function, and may be derived from the study of line profiles.

Astrophysicists usually assume first the physical nature of the motion (such as rotation), then compute the broadening function, and finally compare the observed profiles with those that would be expected from the broadening function. In this way we are able to infer what is the nature of motion which broadens the lines in the geometrical sense. It is apparent that the so-called macro-turbulent motion is far from turbulence as we understand it in the laboratory.

— F. KAHN:

Let me describe a method to estimate the Mach number we can expect for micro-turbulence, in the sense that we have heard it defined. I proceed from the following principle: If we have turbulence at a given Mach number, then energy must decay, and must be removed quickly from the gas, otherwise the gas will heat up and reduce the Mach number. In the astronomical case, the energy is removed by electromagnetic radiation from the gas. The turbulence energy decays to thermal energy of the gas atoms, which can't radiate it away directly, but must pass it on to the electrons by elastic collision. Then the electrons excite the gas atoms by inelastic collision, which leads to the radiation. The rate at which the electron gas gets heated determines how quickly you can cool off the gas, because once the electron gas is warm enough, there is no difficulty in the electrons exciting the atoms to radiate. Therefore the rate at which the electron gas is heated must be large enough

to balance the rate at which decay of turbulent energy heats the gas. So, we compute these two rates, and equate them.

First, with motions of typical velocity  $U$ , atmospheric density  $\varrho$ , and scale length  $L$ , the rate of decay of turbulent energy is given by  $\varrho U^3/L$  per unit volume. In an atmosphere consisting predominantly of hydrogen, with concentration  $N$  and mass  $M$ , the rate is  $NMU^3/L$ . Next in the simple encounter of an electron and an atom, we can expect the increase in the square of the electron's speed to be of the order of the square of the speed of the atom; viz.,  $m\Delta(v^2) \sim mkT/M$ .  $m$  is the electron mass;  $T$ , the temperature;  $N_e$ , the electron concentration;  $S$ , the electron-atom elastic cross-section. Then the elastic collision rate between the electron and atoms is:  $NS(kT/m)^{1/2}$ . Combining these expressions, we find that the rate at which the electron gas picks up energy is:  $N_e NS(kT/m)^{1/2}(mkT/M)$ . Setting  $a^2 = kT/M$ —thus  $a$  is almost the speed of sound in the gas—and equating turbulent dissipation to heating of electron gas, we have

$$NMU^3/L \leq N_e NS(mM)^{1/2} a^3$$

or

$$U^3/a^3 \leq N_e SL(m/M)^{1/2};$$

the inequality sign denotes that turbulent heating of the atom gas must not exceed the rate at which energy can be transferred to the electron gas. This result can be connected with micro- and macro-turbulence fairly easily, because we speak of micro-turbulence in a particular spectral line when the length  $L$  — the scale of the turbulence — is such that there are many turbulent elements within a length corresponding to unit optical depth. If  $\varepsilon$  is the fractional number of atoms considered (relative to hydrogen) in producing the spectral line studied, and  $\alpha$  is the absorption cross-section per atom, we thus require, for micro-turbulence:  $(\varepsilon N\alpha)^{-1} \ll L$ . Thus the above condition on  $U/a$  can be written

$$U^3/a^3 < (N_e S/\varepsilon N\alpha)(m/M)^{1/2} = (XS/\varepsilon\alpha)(m/M)^{1/2},$$

where  $X$  is the degree of ionization. If this condition is not satisfied, the gas must heat up until the Mach number,  $\sim U/a$ , drops sufficiently for the relationship to be satisfied.

For a rough estimate, we take  $X \sim 10^{-6}$ ,  $\varepsilon \sim 10^{-5}$ ,  $S \sim 10^{-16}$ ,  $\alpha \sim 10^{-17}$  and obtain  $U^3/a^3 \sim 1/43$  or  $U/a \sim \frac{1}{3}$ . The value might differ from this, depending upon the cross-sections and the abundance of electrons.

— M. J. SEATON:

What happens if electrons and atoms have the same kinetic temperature — there is then no interchange of energy.



— F. KAHN:

The calculation would be better if the electron gas were considerably cooler than the atom gas. The formula gives the maximum rate at which the energy will be transferred.

— R. N. THOMAS:

Several years ago, KROOK, BHATANAGAR, MENZEL and I tried to see whether we could maintain a steady-state in a pure hydrogen atmosphere with  $T_e \neq T_k$ . ( $T_e$ ,  $T_k$  being electron and atom kinetic temperatures.) We looked for a steady state by equating transfer of energy from atoms to electrons by elastic collision, to energy radiated from the gas, taking into account departures from thermodynamic-equilibrium distribution functions. We had to go to  $T_k \sim 10^7$  before we could get as large a value as  $1000^\circ$  for  $T_k - T_e$ . So, it would seem to me you must take  $T_e = T_k$  in your assumed circumstances.

— H. LIEPMANN:

I am worried, because this dissipation law is correct only for low speeds for which there is no coupling with an acoustic field. Dimensionally  $U^3/L$  may be reasonable, but there could be a dimensionless coefficient of order  $M^{12}$ , for example, when Mach number,  $M$ , exceeds unity. Second, I do not understand why the gas should not heat up; if you have a normal relaxation time, the two kinetic temperatures should become equal.

— F. KAHN:

I do not think that the two kinetic temperatures necessarily are equal. I agree, however, that all kinds of things go wrong when  $U/a$  is large. I would not care to make estimates under those conditions.

— H. LIEPMANN:

I would prefer to see you write the complete equations of motion with the radiative terms in non-dimensional form, and then discuss the relation between the order of magnitude of the various terms. In this case, such a parameter as you introduced must occur, but it can't be the only one; there must be a term corresponding to turbulence dissipating directly into sound.

— R. N. THOMAS:

This goes back to an approach MOYAL and UBEROI made some years ago (MOYAL: *Proc. Camb. Phil. Soc.*: **48**, 329 (1952); UBEROI: *Proc. Camb. Phil. Soc.*: **49**, 731 (1953)), and CLAUSER discussed something along these lines yesterday. I would agree that this would be the most satisfactory approach to the general problem of energy dissipation from a generalized « turbulence » and its relation to electron and atom kinetic temperatures.

— F. H. CLAUSER:

You have assumed that the limiting process is the transfer of energy from atoms to electrons. But suppose the turbulence is buried deep within the atmosphere, so that even though you transfer the energy to the electrons and produce radiation, the radiation just bounces around, being absorbed and re-emitted. Isn't the rate-limiting process the ability of the radiation to diffuse to the surface and escape?

— F. KAHN:

You have raised a more general problem. I was talking only about the region where the spectral lines are formed, near the surface of the star, well above the photosphere. Any radiation generated there will escape; or, rather, any thermal energy turned into radiative energy is unlikely to be turned back into thermal energy.

— R. N. THOMAS:

I disagree strongly. If you want to talk about energy dissipation by radiation in the lines, the biggest problem is the transfer problem. While the rate of radiative loss is certainly proportional to the rate of inelastic collisional excitation of atoms by electrons, the proportionality factor depends upon a solution of the transfer problem, and can be much less than unity. (Indeed, if you make the LTE assumption on the source-function, it vanishes.)

— F. KAHN:

All I really want to say, is that the rate of transfer of energy to the electrons places an upper limit on the rate at which you are allowed to heat the atom gas by turbulent dissipation. You are saying that one should introduce a few more factors to make the inequality stronger. If you don't let the radiation go directly away, you heat the gas, and drop the Mach number of the turbulence. I think that what is really called for is more refined calculation.

— M. J. SEATON:

It is clear that Kahn's calculations represent an extreme upper limit on the energy transfer; in fact, it would probably be several orders of magnitude less. If you consider that electrons and atoms have a small difference in temperature, then  $\Delta T$  rather than  $T$  enters the equation. But this  $\Delta T$  would be very small. What has been done in Kahn's computations, has been to look at the energy transfer one way only, instead of looking at all the energy gain and loss processes. There are collisions leading to an energy transfer back the other way; and it is only when there are differences in the mean energies of the particles that you have a net transfer.

— W. H. MCCREA:

A question on Spiegel's discussion of *31 Cygni*. Can the aerodynamicists say what would determine the scale of the clouds in an extended stellar atmosphere if they be considered to be some form of turbulence?

— E. SPIEGEL:

WILSON and ABT studied the effects of the decrease of the intensities as you go into the eclipse and therefore made some inferences about density gradients. The figure I quoted for «cloud size» is of the order of magnitude of the scale heights, in those extended atmospheres. This is about the only relevant physical parameter I could suggest. Another aspect is that many people don't like to think of motion in the K atmospheres as clouds; for example, in one paper MACLAUGHLIN refers to a network of prominences—and there might be some magnetic or other phenomena which determine the scale. That is a very difficult theoretical question to decide, but a very exciting one.

— A. UNSÖLD:

This is a viewpoint about which we should remind aerodynamicists quite generally. If looking at a «point» on an astronomical object we observe a spread in velocities, then of course we see at the same time a lot of objects lined up along the line of sight. These may be clouds, or, for instance, something like the prominences on the sun, which have nothing whatever to do with each other. And then it becomes senseless to speak of a continuous fluid motion. Such things can happen in astrophysics, but are usually not considered in aerodynamics.

THOMPSON will now summarize this session from the standpoint of the aerodynamicist; *i.e.*, what, from this astronomical material, appears to be of interest in aerodynamics.

— W. B. THOMPSON:

It has been assumed that I will summarize for you what I have understood, and then you can safely assume that the other hydrodynamicists have understood at least that much.

From the discussion so far it appears that the astronomer divides his interests into something called micro-turbulence—and something called, with even less justification, macro-turbulence. As I understand it, and I may be very unkind, the suggestion already advanced—that micro-turbulence is an artifice introduced to correct an erroneous theory of line formation—is not completely excluded. We have been assured by UNSÖLD that this is not the case. There do exist valid theories of line formation which demand the existence of micro-turbulence. What he means is not the sort of analysis described by

PECKER and THOMAS, but a synthetic theory; the construction of a model atmosphere from which one predicts line shapes, and after the introduction of micro-turbulence secures very satisfactory agreement with observations. We have not yet, however, been exposed to the physical assumptions that have gone into this theory, and clearly it is not a complete *a priori* calculation based only on the microscopic properties of matter, for one does not know enough about atomic cross-sections—or even about the relevant processes involved to start from first principles. It is quite clear that at some stage simplifying assumptions have been made, Saha and Boltzmann equations, or local thermal equilibrium, and it would be interesting to see just what the simplifying assumptions are, and what physical arguments underlie the selection of these. These I am sure are familiar to all the astronomers present: they are not familiar to me, nor, I suspect, to some other of the physicists and aerodynamicists. It would be very enlightening if we could be shown the physical arguments underlying the theoretical calculations which have made inevitable the introduction of micro-turbulence.

Micro-turbulence is not so worrying now as it appeared at one stage of the discussion—when I and several others were under the impression that the evidence required supersonic micro-turbulence, which would have been rather hard to swallow. However UNSÖLD has presented the general conclusion that it is always subsonic—although the velocities are large.

A second interesting feature is that we know the scale of micro-turbulence. Since it is involved in the curve of growth, widening the line core, its scale is less than the mean free path of radiation, or the optical depth  $\tau = 1$ . On this small scale, the high, almost sonic velocities required present serious difficulties, but I don't believe they are insuperable, particularly when you recall that the visible layers lie on top of a much hotter substratum. Scales: the impression I have is that  $\tau = 1$  corresponds to a modest length in the observable parts of the star, say 100 km.

(*Ed. Note:* There followed an interchange between THOMPSON, UNSÖLD, THOMAS on this question of scale, making the points: For strong lines, there is a variation of a factor  $10^4$  in scale over which the different parts of the line are formed. The distance  $(100 \div 1000)$  km is a reasonable estimate for a length corresponding to  $\Delta\tau \sim 1$  in the continuum, in the wings of strong lines, and in weak lines; so long as one does not consider stars with extended atmospheres.)

— W. B. THOMPSON:

A final observation about micro-turbulence. It seems that the only way of getting at the physical structure of things of this scale in the stars is by making model atmospheres and exploring the consequences of specific models. On the other hand, PECKER and THOMAS have suggested that for the partic-



ular case of the sun, for which much more information is available, it may be possible to extract a good deal of detailed information about the physical system by examining lines directly, rather than by working from model atmospheres. Thus the sun is quite a different object for study than the stars, and I would have thought that in the present state of our ignorance it might be a good idea to concentrate on understanding the hydrodynamics of the sun, which I gather is fairly simple-minded, well-behaved star.

Now to macro-turbulence. If hydrodynamicists object to micro-turbulence—since in fact what is involved may be a superposition of unlinked sound waves rather than the turbulent field in which the velocity components are tightly interlinked, as CLAUSER described—how much more exception should they take to the term macro-turbulence, since here the scale is large—much greater than 1000 km. May I observe that the scale of the hydrodynamicists' turbulence contains parts which are independent of the geometric scale of the objects producing the turbulence. The scale will ultimately be determined by dissipative processes, nothing else. This is the meaning of turbulence as used by the aerodynamicists. It is a velocity field, which is coupled to itself through non-linear effects, in which energy is cascading from large scale phenomena into smaller and smaller scale phenomena. The scale with which you start is of course determined by the geometrical size of the atmosphere of the object which you are looking at, the final size of the small eddies is determined by dissipative processes and is the same in the laboratory as it is in the star, in so far as physical conditions are similar. That one sees micro-turbulence in those stars where macro-turbulence is also observed, suggests there is some passing down the scale—that there is some connection between these two things. But I do think they can be considered as distinct phenomena, with no necessary connection. It is just as well, because there seems to be quite a difference between them. In particular, macro-turbulence for some moderately pathological stars can be violently supersonic. That is, the Mach number is very great indeed. Miss UNDERHILL described this morning what is known about macro-turbulence in stars. It seems that a typical star exhibiting macro-turbulence has a large gaseous envelope around it, and seems to be a little unhappy in various ways—these stars are not steady, they seem to suffer from some sort of astronomical indigestion. So it is maybe not too surprising that we see large scale motion. Observe that the Mach number is very much greater than 1, but that the temperature used to estimate it was, of course, the temperature of the outer thin cool layers of the stellar atmospheres. Because of the possible scale of this motion, which can be anything from something greater than a few thousand km to something comparable in size to the entire star that one is looking at, from a hydrodynamic point of view this Mach number may be completely irrelevant.

Hydrodynamic behaviour on this scale is determined not by the thin cool

atmosphere, but by the hot underlying matter of the star for which I suspect the Mach number to be less than one. The Mach number seems to me irrelevant to the macro-turbulence because the Mach number is with respect to the temperature of the thin, cool, skin on the surface of a star, and this surface temperature is completely irrelevant to the actual processes determining the motions which may occur in deeper layers. That of course can scarcely apply to these extremely tenuous atmosphere which have a very large volume indeed. As far as I can see from the evidence of *31 Cygni* presented by SPIEGEL, the atmosphere can be very much larger than the star itself; so that the transparent region you see is very extended and cool. There this consideration cannot apply. On the other hand, if there is a strong magnetic field in such a thing, the relevant Mach number should be with respect to the Alfvén speed, not to sound velocity and again it may be very much reduced.

— A. UNSÖLD:

Let me try to clarify some points, which have been brought up, in a kind of second approximation.

I will consider first the question of macro-turbulence. One of the most exciting statements for the astrophysicist in this morning's lecture by Miss UNDERHILL—based on new observation at the Dominion Astrophysical Observatory—was that the macro-turbulence that is observed in hot supergiant stars is always of the same order of magnitude as the changes that one measures in the radial velocity of the whole star, as a function of time, over longer times. That is for the first time a really convincing indication that these velocities are not connected with the rotation of the star, but are due to really irregular motions, which comprise considerable parts of the star. We have somehow to imagine that considerable parts of the stellar atmosphere move up and down in a rather irregular way. The detailed mechanism of course is far from clear. We may imagine that it has something to do with pulsation in higher modes. And that, of course, would come into perfect agreement with the viewpoint raised by THOMPSON, that relating these speeds to the velocity of sound for the temperature of the atmosphere in the usual way may have no sense. I think this is an important point which was not clear so far and which we should fix as a real result from this meeting.

Then comes the other question of micro-turbulence, where you did not quite feel satisfied about the explanation of the physical foundations. Perhaps I should say a few words more on these. I must attempt to explain briefly and in simple words a type of work which in fact is extremely circumstantial and lengthy, as I said this morning. Let us begin by assuming that we know the effective temperature, the surface gravity, and the composition of the star. Then we try to calculate the structure of its atmosphere; that is, how the

temperature and the pressure depend on the depth. To begin with we assume a perfectly static atmosphere.

— W. B. THOMPSON:

You make essentially a calculation based on hydrostatic equilibrium. That must involve some assumption about transfer of heat. What other assumptions?

— A. UNSÖLD:

It is assumed that the energy is transferred in the higher layers entirely by radiation. For the deeper layers of the cooler stars convection is important too. These are processes which we can describe with sufficient accuracy. Then, if we know the dependence of temperature and pressure on depth, we can calculate for instance how the number of sodium atoms in a particular atomic state depends on depth. Next, we calculate the absorption coefficients.

— R. N. THOMAS:

You are assuming certain things when you calculate how occupation numbers depend on depth. Maybe you could mention the assumptions, and whether you have investigated their validity for the situation which you are examining.

— A. UNSÖLD:

We assume the Saha equation and the Boltzmann equation. Perhaps I should state the limitations of the procedure afterwards. I hope they become clear then. We calculate the atomic absorption coefficients for various lines as a function of depth. These are calculations which one can do nowadays fairly well from quantum theory, at least for a sufficient number of atomic states. Then we can calculate the curve of growth for these lines. Still without assuming turbulence. And now comes the process of fitting our calculations with the observations: We have to check on the one hand the temperature, and on the other hand the surface gravitation. Certain lines are more affected by temperature, and others by the pressure, which essentially depends on the surface gravitation. So we try to fix these two points by combining various observations. In that procedure the abundance of individual elements does not come into play. Then comes our important point—how to get the turbulence. First, we must draw the curve of growth in dimensionless units. Let us plot the measured equivalent widths of the lines divided by what one usually calls the « Doppler widths »; that is, the width of the absorption coefficient caused by the combined action of any motions which are there, which is thermal motions plus what we call turbulence. The abscissa is essentially the concentration of the atoms times the transition probability. If these quantities are plotted logarithmically, the linear part of the curve of growth becomes a 45° straight line; then comes the flat part, and then comes the damping part, with

half the inclination of the first part. Now we take an element, which has at the same time very weak lines and lines of intermediate strengths. For these lines we know the ratio of the transition probabilities and so the distance between their points along the abscissa. Next we attempt to bring our « empirical » curve into coincidence with the « theoretical » curve of growth by shifting in horizontal and vertical directions. The horizontal shift determines essentially the abundance of the element while the vertical shift gives the ratio of the real Doppler-width (thermal motion plus « turbulence ») to the thermal Doppler-width alone.

— W. B. THOMPSON:

That gives you  $\Delta\lambda_D$ . Now you must have something to produce  $\Delta\lambda_D$ , and you invoke turbulence rather than departures from Saha or anything else.

— A. UNSÖLD:

Let the distribution of velocities  $\xi$  along the line of sight be  $\sim \exp [-(\xi/\xi_0)^2]$  for the thermal part alone and  $\sim \exp [-(\xi/\xi_t)^2]$  for what we call turbulence alone. Then we determine the ratio  $(\xi_0^2 + \xi_t^2)/\xi_0^2$ . The temperature must be known from other parts of the analysis. As I said the horizontal shifts of curves of growth determine essentially numbers of atoms in certain atomic states and so one can use ionization—and excitation—equilibria for determining temperature and pressure. The essential trick in this type of spectral analysis is that one knows beforehand from a general study of the subject, which is of course a matter of some experience, that one line depends chiefly on temperature, another line depends chiefly on pressure. Also, one knows that the flat part of the curve of growth depends strongly on the velocities and then one combines the different observations. It is the experience with each one of our students that he complains first that a stellar spectrum has several hundred lines which he has to measure, and when he finally comes to the end of the analysis, he complains that this star has by far too few lines to determine all the parameters of the atmosphere. The essential point is that one uses one and the same set of plates for determining *all* the parameters—the effective temperature, the surface gravitation, the abundance of all the elements, and—if necessary—the turbulent velocity.

W. B. THOMPSON:

You have, of course, given this explanation with great care and it is very much like the other explanation that we have heard of the curve of growth. One point which still leaves some doubt in my mind, is the determination of the temperature itself. This has been done from things like Saha using the equation for ionization equilibrium, or relative line intensity and the Boltzmann equation. How sure are you of the validity of these determinations of the temperatures?



— A. UNSÖLD:

It is essential that we make clear about what temperatures we are speaking. In all our work we use as our characteristic parameter of a star the so-called effective temperature, which is defined as representing (in connection with the Stefan-Boltzmann law) the total energy flux per  $\text{cm}^2$   $\pi F = \sigma T_{\text{eff}}^4$ . With this parameter we can calculate, using the theory of radiative equilibrium, the real temperature at every point in the atmosphere. Here we assume that we know accurately enough how the radiative transfer is done.

Now recently THOMAS, PECKER and others have put more emphasis on the investigation of the higher layers of the solar and stellar atmospheres, where the assumption of local thermodynamical equilibrium becomes worse and worse.

Let us try to get some idea about the boundary between the two alluded domains!

At an optical depth  $\approx 1$  (for the continuum) in the atmosphere, a fictitious observer would receive almost as much radiation from the outside as from the inside. Nearer towards the top of the atmosphere the radiation coming from outside becomes less and less and we receive radiation only from the lower hemisphere. So, if we go up high enough, we can certainly have significant deviations from thermal equilibrium. The question is whether these thin uppermost layers still contribute appreciably toward the production of the stellar spectrum. In general the smallest optical depth which is important for the explanation of the continuum and the equivalent widths of lines will be about 0.05. At such depths for the continuum, however, the optical depths for the stronger lines are still quite large and if one has some mechanism working towards establishing thermal equilibrium, *i.e.* exchange between different light-quanta, then just this radiative transfer will help a great deal towards establishing local thermodynamic equilibrium. So for these layers the deviations from the Boltzmann equation (in general) are expected to be fairly small and for the Saha equation quite moderate. K. H. BÖHM has made some time ago (in an article for the new *American Handbuch*) estimates how, *e.g.*, in the outermost layers of the sun the ionization of iron will deviate from the Saha formula and it turns out that this effect is in general not large. In any case it will not affect the spectroscopic determination of the micro-turbulence. The matter becomes, of course, quite different if we go in the sun to higher layers in the chromosphere or still more in the corona. These are places where THOMAS likes to live and there things may be quite different. But these regions contribute little to the ordinary Fraunhofer spectrum which one observes on the solar disk or in stars.

— W. B. THOMPSON:

In determining the turbulence do you use the weak lines?

— A. UNSÖLD:

In order to determine the turbulence one must have lines which fit on the flat part of the curve of growth. You may see easily how strong these lines must be. Namely, the equivalent widths of the lines there is about 4 times the Doppler width. We saw that for purely thermal motion the Doppler widths for the metals are a few hundreds of an Ångström; so the mentioned lines come into the order of roughly one-tenth of an Ångström.

— W. B. THOMPSON:

Are such lines formed in that part of the atmosphere in which you are suspicious about thermal equilibrium?

— A. UNSÖLD:

No, these lines are formed in practically the same layers as the weaker lines and the greater part of the profiles of the stronger lines. Lines lying on the flat part of the curve of growth have almost rectangular profiles and their equivalent widths are determined by the points where their depth is  $\sim 50\%$ . These points of the line profiles however originate from quite intermediate layers in the atmosphere. So, I think, the measurements of turbulent velocities (within an accuracy of  $\sim 10\%$ ) should not be affected by deviations from thermal equilibrium.

— R. N. THOMAS:

Let me try to put the points at issue into focus, recognizing the presence and prejudice of three kinds of interest at this symposium: A) an astronomer who is interested primarily in determination of chemical composition of the stellar atmosphere, and considers the presence of non-thermal velocity fields an unfortunate complication whose presence is to be eliminated from the analytical process as expeditiously as possible; B) an aerodynamicist who hopes to extend the range of his experience outside laboratory aerodynamics, thus is concerned with details of aerodynamic phenomena; C) a hybrid who is interested in the non-LTE phenomena attending a mixed situation of radiative transfer and «dissipating» velocity fields, thus wants details on everything. Then we must recognize that the methodology discussed by UNSÖLD is essentially aimed at satisfying A); it is essentially based on the supposition that the only effect of non-thermal velocity fields lies on the frequency-dependence of the absorption coefficient, such velocities have no effect on thermal structure of the atmosphere nor on atomic concentrations. That is, two procedures must be valid: 1) temperature distribution can be computed from radiative transfer of energy only, *no* energy dissipation from either «micro- or macro-turbulence» being allowed; 2) all occupation numbers of energetic states can be

computed from thermodynamic equilibrium distribution functions, using the temperature computed from 1).

1) is certainly violated in regions where electron temperature,  $T_e$ , increases outward; for there some non-radiative energy input, presumably aerodynamic dissipation must occur. For the sun, this outward increase in  $T_e$  begins near  $\tau$  (continuum)  $\sim 0.01$ . However, from the type analysis described by UNSÖLD, there comes no suggestion of velocity fields, in this region, larger than those he has just discussed as having negligible effect. So, one would conclude—using only the UNSÖLD type analysis—that there is no departure from validity of 1), and this conclusion would be erroneous. Indeed, the methodology of the same LTE approach has been applied by the UNSÖLD, and other, groups to obtain a monotonic outward *decrease* in  $T_e$  in the same atmosphere regions where other analyses, based on less-restrictive assumptions, show an outward *increase* in  $T_e$ .

2) must certainly be violated where 1) is violated, so it remains to compute the opacity in each line to show where it is formed, relative to the region where 1) is violated. But since 2) is violated everywhere above  $T_e(\text{min})$ , at least, such opacity calculations can only be made on a non-LTE basis. We have shown that such non-LTE calculations sometimes increase, by several orders of magnitude, the opacity computed from the LTE approach; so the latter will often seriously err in predicting what regions suffer from the non-LTE effects, even if they had been successful in predicting where 1) is violated. Also note that 2) may be violated even in regions where  $T_e$  does not suggest aerodynamic dissipation, the violation coming from anisotropy of radiation field. PECKER discussed yesterday empirical evidence for such failure for intermediate and weak lines, of the type considered by UNSÖLD. We have shown theoretically and empirically such failure, for strong lines.

Further, note that the atmospheric range over which a line-profile is formed may be enormous, more than  $10^4$  in optical depth for a reasonably-strong line. In consequence, it may well be that in certain cases the curve of growth, based on total absorption in the line, averages things out so well that it indeed « suppresses » the value, and effect, of such things as velocity fields, non-radiative dissipation, and non-LTE effects—leaving only a reasonably-good measure of chemical abundance. But then, it is hard to place much reliance on physical interpretation of « velocity parameters » derived from it. The point is, we require, before passing final judgement, much more investigation of the curve of growth from the standpoint of including at the outset the presence of all these neglected factors. Again, PECKER has referred to the preliminary work at Meudon along these lines.

So to some of us, it has appeared that the information required by groups B) and C) above comes best from analysis of line-profiles, particularly the central

regions. Then, we must look carefully into two questions: the validity of the methodology used in the analysis of line-profile; and the question of computing the opacity, to say where the line is formed, relative to the continuum and other lines. A good example is the large amount of current work interpreting the central profile of Ca  $H$  and  $K$  terms of turbulence, non-LTE effects, etc.

— A. UNDERHILL and A. UNSÖLD:

Yes, but this doesn't affect our equivalent widths. In the central part of the line, profiles can be measured only very roughly, due to plate grain and lack of resolving power. And, we don't discuss  $H$  and  $K$ ; we talk about FeI, TiI, TiII, CrI and things like that.

— M. MINNAERT:

If you take photoelectric records such as are obtained, *e.g.*, at the McMath-Hulbert Observatory—take Fe, Ti, Cr if you like—you will find the curves are quite smooth. What you refer to are old-fashioned photographic methods, modern methods are photoelectric. Theories must be adapted to modern methods, and not to old-fashioned methods.

— A. J. DEUTSCH:

I should like to give my impression of why it is that there are such strong disagreements on this subject among astrophysicists. I think the working philosophy for the astronomer who actually does a curve-of-growth analysis, perhaps of the kind that UNSÖLD just described, has been at least historically, something like this. He is perfectly content to start with the thermal Doppler widths. When he plots his equivalent widths in a curve of growth, he then finds that he gets one curve of growth for iron, and a slightly different curve of growth for titanium, and still another curve of growth for sodium, and another one for calcium, and so on and so forth. And at this point he asks what is the least complication that he can introduce into the theory of stellar atmospheres which will enable him to reconcile these apparent discrepancies. He comes up with the answer that he can introduce a single new parameter which has the dimensions of a velocity. The Doppler width  $\Delta\lambda_D$  associated with this velocity replaces the various thermal Doppler widths, and is the same for all the atoms which are considered. Then all the observed points move nicely on to the same curve of growth.

Now THOMAS is going to explode in a minute, and I think it needs to be added that there are astronomers—and THOMAS is by no means the only one—who say that this is no true, that even after he has made this adjustment he will get significant systematic differences between different atoms. Now this is where the difficulty lies. Some astronomers insist that the present theories



are entirely adequate to satisfy the observations at hand; and there are some extremely competent astrophysicists who maintain this position with respect to most of the lines in the solar spectrum. And there is another group who say, no, if we use the best photometric measures we have and the best of the other relevant data, we still get discrepancies which cannot be reconciled with any choice of the Doppler parameter; we must change the source function.

Now, I should like to point out that in addition to the question of the precision of the photometry, which may be involved here, there is also some question as to what should be used for the scale of abscissae. The doubtful parameter is the oscillator strength, or *f*-number. *f*-numbers in astrophysics play a critical role. They have done very notorious things to us in the past. You have the uncomfortable feeling that they are still doing very unpleasant things to us at the present time. The answers that we get from the curve of growth may depend very sensitively on the numbers that we take for the oscillator strengths. These are difficult to determine precisely. Some astronomers prefer to take their oscillator strengths from one source, and some to take their oscillator strengths from another source. Some astronomers assert that by using a more suitable set of oscillator strengths, it is possible to remove the discrepancies that are cited by the people who insist on the necessity of changing the source function. I cannot take a position on this question; I do not know. But I suggest that this may be a fair appraisal of the reasons for the wide disagreement which you will find among astrophysicists at the present time, about the necessity of abandoning the relative simple equilibrium model which most astronomers have been content to use in the past.

— A. UNSÖLD:

I agree with DEUTSCH on the viewpoint that one must be extremely conservative in using oscillator strengths. Then we have been frequently talking about deviations from thermal equilibrium. No doubt such deviations exist; but opinions are divided on their importance. In any case we should make clear that we are dealing with two quite different problems. Imagine first a perfectly quiet atmosphere in purely radiative equilibrium. In its outermost layers there is no radiation coming from outside, and that will lead to deviations from thermal equilibrium. On the other hand, if we have an atmosphere with motions (from whatever cause), their velocities will increase outwards and we get energy transfer also by mechanical motion, *e.g.*, by dissipating shock, or hydrodynamic waves, etc. Such effects may produce again deviations from thermal equilibrium which may be rather different from these mentioned first. It might clarify the discussion if deviations from thermal equilibrium having quite different physical background would be distinguished from each other.

(Ed. Note: For a discussion in terms of such a distinction, cf. the PECKER-THOMAS paper, section on the two categories of source-function for a 2-level atom.)

— G. ELSTE:

May I give an example of improved «classical» methods: *i.e.* no departures from LTE and no turbulence has been used in the model atmosphere of  $\tau$  *Scorpii* which ALLER, JAGAKU and I were looking at some years ago. I call it an improved method because not only the abscissae of the curve of growth but also the run of  $\log W_\lambda \lambda$  as a function of  $\log \tau$  was calculated theoretically for Si III and Si IV. As a result the observed points agree very well with the calculated curves leading to the same Si abundance. But look

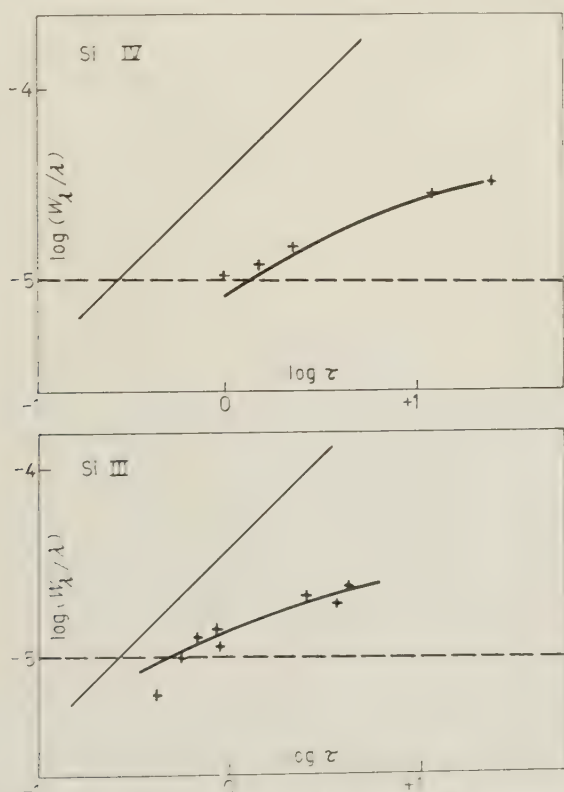


Fig. 2. Curves of growth for silicon in  $\tau$  *Scorpii*.

at the position of these curves of growth. There exists quite a difference between Si III and Si IV. While the Si III-curve does not differ much from the common Milne-Eddington curve, the Si IV-curve runs below it and in the region where one expects «weak» lines it does not at all reach the 45 lines.

This behaviour can be understood by the rapid increase of the number of Si IV-ions in deep layers where these lines are formed and saturated, and above this layer there still exists continuous emission. This note may be a warning, because there are cases in which the different positions of the points for different ions are interpreted as difference in turbulence velocity.

— R. B. LEIGHTON:

I have a question concerning stars with extended envelopes. May we assume that the objects that have been studied, that show large turbulent velocities, are typical for *all* supergiants? Or have we merely looked at a limited sample, which could give us misleading results?

— A. UNDERHILL:

That is a very difficult question to answer because very few supergiants have been observed. There is nothing about those supergiants which have been observed that is particularly different from any other supergiant; but each supergiant is a bit of a character of its own. I think you could conclude that the observations give a reasonable representation of what any supergiant might be expected to be. You just cannot make statistics from a handful of observations; though astronomers try very hard most of the time.

— M. J. SEATON:

I am still not clear on the precise attitude that PECKER and THOMAS hold on these questions. Yesterday we had some terrible warnings about all the uncertainties, attending use of the «standard» methodology and I think it would perhaps be useful if PECKER and THOMAS could make it plain just what they do accept. From a question asked this morning, I almost had the idea that the whole concept of micro-turbulence was rejected, or at least that source function uncertainties are so large that no information about micro-turbulence can be obtained. But is this really their point of view?

— R. N. THOMAS:

Our viewpoint is very simple: Do not take literally the result of any observational interpretation unless this is made on the basis of the most complete physical theory you can construct. UNSÖLD's book on stellar atmospheres is still, to me, the best that exists—because he worked very hard to insist that one put all possible physics into astrophysics. We are trying hard to extend this viewpoint into fields he did not consider, the non-LTE and aerodynamic-dissipation aspects. The detailed theoretical results obtained thus far are exploratory, and limited in scope—mainly limited to the central regions of strong lines—but they have introduced strong changes over results based on the LTE approach. Relative to the micro-turbulence derived from curve-

of-growth studies, I would only repeat my remark of a few minutes ago on the possible «averaging-out» character of the gross curve-of-growth approach, its insensitivity to effects found in regions its result presumably cover, and resultant suspicion on the physical meaning to be attached to parameters derived *a posteriori* from it. I see little point in proceeding to construct aerodynamic theories to explain an inferred velocity field, until I am sure that the existence of such a velocity field is consistent with the basis upon which I have derived the theory used to infer the presence of the velocity field.

— A. UNDERHILL:

The rather large motions that appear in all types of stars do not differ greatly. Different amounts of radiative energy flow through the stellar atmospheres, but you get about the same magnitude of velocities. It is to me extremely interesting that the magnitude of the velocity appears to depend far more on the size of the atmosphere than on the absolute value of the energy flowing through it. Practically all the discussion today has been concerned with astronomical micro-turbulence; I wonder if I may infer that no really interesting aerodynamical problems are posed by this other aspect, that seems to me a rather interesting field?

— F. H. CLAUSER:

Frankly, I am not very clear on what you are asking. I think that those of us who have been associated with turbulence in the laboratory feel that turbulence is not a definition—it is not an invention—it is not a catch-all. It exists as a reality and it has an existence that is forced upon us by observations in many different fields under many different conditions for a variety of fluids, and I think that we are interested in the fact that it exists under your circumstances. We find that turbulence exists so universally that it would not surprise us at all if in every star you found turbulence. That you find micro-turbulence that is subsonic, I think has been aptly expressed by the statement: we would be most surprised if you did not. The fact that you find macro-turbulence with very large velocities, again is not surprising to us. Just how much our interest is, I am not quite sure; because it is not clear to me, at least, what you are really measuring, with macro-turbulence.

I have been sitting here thinking about how, in the laboratory could one generate supersonic macro-turbulence; and it suddenly occurred to me that we have a lot of it, every place. For example, supposing that I were to take the ordinary wind tunnel—a simple, ordinary wind tunnel in which the flow comes in at very low subsonic speeds, goes through a nozzle at sonic speeds, and accelerates to supersonic speeds. I put glass walls on it, turn it at a slight diagonal angle so that you get a component in the line of sight, turn a light through it and allow you to analyze only the total light that comes from the



entire tunnel including the supersonic and subsonic portions. And you will get supersonic macro-turbulence. We would no more call this turbulence than the man in the moon, but it appears to me that you would call it turbulence—you would call it macro-turbulence. Now I think we are a little unhappy about this. Because we feel that turbulence is very real, and even though we cannot define it precisely, we find that there is a large range of phenomena under which it is clear cut that it is turbulence.

Now, let me tell you a few things that are not turbulence. For instance, a random sound field. If you were to generate pulsations on the walls of this room, you would get a velocity field in this room, and I do not believe that anyone of us would call it turbulence. If you look out at the surface of the lake and see the surface of the lake moving up and down, we do not call that turbulence. There are a large number of such things that we do not call turbulence. Now, let me tell you some of the things that we do call turbulence. I think one of the most startling things that we find is the following: turbulence, like pregnancy, is all or nothing. There is no such thing as half-turbulence. For example, we used to believe that turbulence could die out, and get finer and finer, so that you just get less and less of it as you went out into the field that adjoins essentially a large mass. But as we got more sophisticated instrumentation, that could resolve in both space and time, we found that the border between the turbulent and non-turbulent parts of flow was very sharp and very distinct. The only reason that you thought that you had less turbulence was because your instrumentation for a small fraction of the time was immersed in a turbulent field. We also observe that in the transition that took place between a laminar flow and a turbulent flow you got bursts of turbulent and bursts of non-turbulent, flow. Then we begin to look more and more to see if we could ever find a case where the turbulence simply died out; and to my knowledge, we have never found such a case. In every case where the instrumentation has been adequate and proper, the boundary between the turbulent and the non-turbulent fields is very sharp and very distinct. And, the sharpness of the boundary seems to be comparable with the smaller eddies; as near as we can tell—the characteristics of the turbulence carry right out to the boundary. Now, if we look at these things optically, say we shine light through the turbulent wake of a bullet, the boundaries at the edge are as sharp and clear as any of the finest eddies that we find. The reason, that I say all this is because we try it with liquids, and we try it with gases—we try it with non-Newtonian fields that do not have linear viscosity laws. We try it with compressible phenomena. We try it under a great variety of circumstances, and we find that turbulence is a very real phenomenon. It is not an invention of ours. It is not a catch-all, just to include anything else you do not know. This is what you appear to be using it as. This makes me, at least, unhappy.

## PART III.

### Spherically-Symmetric Motions in Stellar Atmospheres.

#### A. - Pulsating Variable Stars.

##### Summary-Introduction:

##### Velocity Fields and Associated Thermodynamic Variations in the External Layers of Intrinsic Variable Stars.

P. LEDOUX and C. A. WHITNEY

*Université de Liège and Smithsonian Astrophysical Observatory*

#### 1. - General introduction.

By intrinsic variable stars, we mean those stars which present variations in light, spectrum and radial velocity which cannot be accounted for in terms of purely geometrical or orbital factors; so that we have to appeal to some kind of periodic physical modification of the star.

This class is very large, and it would be quite impossible here to review even briefly the properties of all the different sub-classes (LEDoux and WALRAVEN, 1958; hereafter called reference A); so we shall limit ourselves to one of the best known groups, which comprises the cepheids and the *RR Lyrae* stars. The essential factors which, up to now, have been called upon to explain the properties of the various kinds of intrinsic variable stars, excluding the most irregular or the most violent types (such as novae and super-novae), will be brought up in the discussion of this group.

The cepheids are supergiants of mean absolute bolometric magnitude,  $M_{bol}$ , falling in the range  $-3$  to  $-6$  and of spectral type F to G. In a Hertzsprung-Russell diagram, using  $M_{bol}$  and  $\log T_e$  as co-ordinates, they occur about  $\frac{2}{3}$  of the way from the main sequence to the giant branch, where they form a sequence roughly parallel to the latter (cf. ref. A, p. 572). They comprise stars typical of Populations I (disk and spiral arms), often referred to as classical cepheids, whose periods vary from about 2 to 40 days from the less to the most luminous objects of the class; and stars typical of Population II (globular clusters and spherically distributed stars in the galaxy), which show a concentration in two ranges of periods: 1 to 2 days and 13 to 20 days. Stars in the second group are often designated, after the prototype,

as *W Virginis* stars. Although the basic phenomena are probably the same, there are many interesting physical differences between the classical cepheids and the *W Virginis* stars, at least in the behaviour of the external layers.

The *RR Lyrae* stars, which belong to Population II, have periods between  $\frac{1}{4}$  of a day and 1 day. The exact value of their absolute magnitude, which is roughly of the order of  $0.0 M_{bol}$ , is at present a matter of great concern as it plays an important role in fixing the distance scale of the Universe. Their spectral type falls in a small range around A5. In the Hertzsprung-Russell diagram of a globular cluster, they fill a small definite gap in the horizontal branch. This suggests that any star which, in the course of its evolution, goes through this gap, becomes the seat of this type of variability.

In some of the *RR Lyrae* stars there appears, superposed on the periodic variation referred to above, a very regular modulation of very long period (60 to 1400 times, the main period), which affects both the amplitude and the phase. The resulting variation can be interpreted as a beat phenomenon between two variations of very close periods.

Some cepheids in the range of periods 2 to 3 days present a similar phenomenon, but with a modulation period that is a much smaller multiple (2 to 3 times) of the fundamental period.

The phenomenon of multiple periodicity is perhaps best represented by a group of variables with very short periods, of the order of 0.05 to 0.2 days. Despite the fact that, as far as the periods are concerned, they fall close to the lower end of the *RR Lyrae* variables, they do not probably belong to that class and are sometimes called dwarf cepheids. One of them which has been studied extensively, *AI Velorum*, presents no less than four well marked periods.

A beat phenomenon is also encountered in some of the  $\beta$  *Cephei* stars, a group containing about 12 known members with main periods in the range  $3\frac{1}{2}$  to 6 hours and composed of bright blue stars, B1 to B2, somewhat above the main sequence. However, in this case, the two interfering variations must present some significant physical difference, since one of them is associated with a variable broadening or doubling of the lines while the other does not seem to affect the line shapes. This suggests that the type of motion may be more complicated here than in the case of the other variable stars mentioned above. Note also that the light amplitude is very small in this case.

For well observed classical cepheids and *RR Lyrae* stars, the periods can be defined with a very high precision and are very stable. In some cases, slight secular changes have been found, but no systematic trend has been discovered up to now. In any fairly homogeneous group, there are about equal numbers of stars with lengthening or shortening periods.

With their very short periods, the  $\beta$  *Cephei* stars provide a very favorable case for the study of such secular effects and, indeed, there seems to be a tendency, in this group, for increasing periods.

For the aerodynamicists, let us recall the fundamental characteristics of the variations of a typical classical cepheid,  $\delta$  Cephei (cf. Fig. 1). The light emitted by the star varies periodically, as does its radial velocity, and the two corresponding curves are practically mirror images of each other. This correlation between the two curves extends even to many details particular to individual stars (humps on the ascending or descending branch, etc.) and suggests that there must really exist a close physical relationship between the two.

The amplitude of the light curve varies from about 0.5 m to about 2 m as the period varies from about 3 to 30 days. In the same way, the amplitude of the velocity curve varies from about 10 to 35 km/s. The spectral type, which is related to the temperature and the density, varies in phase with the light. The corresponding variation of the effective temperature,  $T_{\text{eff}}$ , for  $\delta$  Cephei, is represented in Fig. 1e. This is the most direct argument for the existence of physical changes in the star. But one may also recall that the shape of the light and velocity curves, and the phase relationship between the two, exclude any interpretation of the light variation in terms of eclipses. On the other hand, the discussion of the velocity curve as being due to orbital motion leads also to highly improbable results.

An explanation of the cepheids in terms of oscillations of a single star was advocated for the first time in a paper by SHAPLEY in 1914. At that time a non-radial oscillation such as that corresponding to a spherical harmonic of degree two was mainly favored because it was thought easier to excite; for instance, by close passage of another star. However, EDDINGTON noted, that this last argument is not very significant and, on general grounds, that

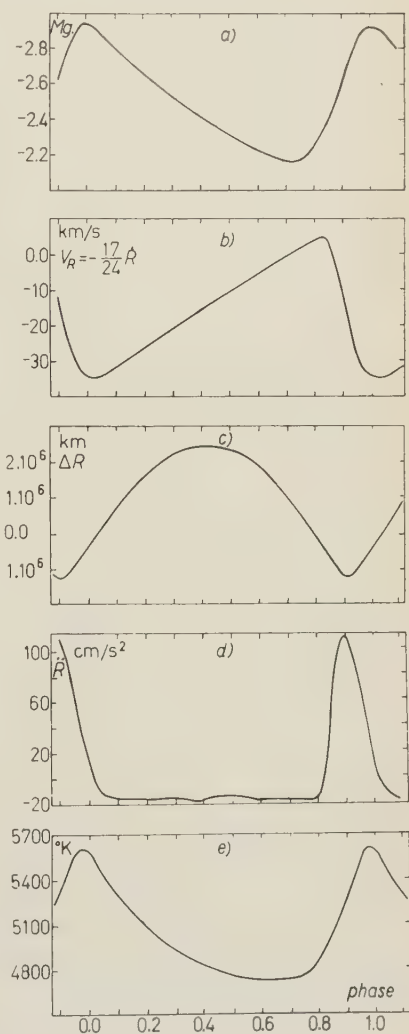


Fig. 1. — a) Light curve, b) velocity curve, c) radial displacement, d) acceleration, e) effective temperature, for  $\delta$  Cephei.



one would expect that a purely radial pulsation would be the easiest to maintain. He also pointed to the general spatial symmetry of the phenomenon, an argument which has been confirmed recently by the correlation, found in well observed cepheids, between amplitudes and periods. This result could hardly be expected for any other type of symmetry of the oscillation than a purely spherical one. Thus at present, it is generally believed that, at least in cepheids and probably in the *RR Lyrae* stars, the variations are due to radial pulsations: the star expands and contracts periodically.

In that case, if we change the sign of the radial velocity curve and multiply its ordinates by a factor  $24/17$  to correct for the averaging over the visible hemisphere and the limb darkening, we get the rate of variation of the radius of the star,  $R$ , which, integrated with respect to the time, yields the variation  $\delta R$  of the radius (cf. Fig. 1c). In classical cepheids, the relative amplitude  $(\delta R/R)_A$  varies from 0.05 to 0.1 at most, while in *RR Lyrae* stars it is a little larger, and lies in the range 0.1 to 0.2. In *W Virginis* stars, it would seem that  $(\delta R/R)_A$  may reach appreciably higher values, of the order of 0.3 or larger. However, as will be pointed out later, the evaluation of  $\delta R$  is not quite as straightforward for these stars.

On the other hand, the derivation of  $-V_R$  with respect to the time yields the value of the acceleration in the atmosphere (cf. Fig. 1d). The results show that these layers are submitted to a strong upward force only for a short time; during most of the period, they fall regularly under a practically constant downward force.

On a simple adiabatic theory, one would expect the star to be hotter at maximum compression. With the usual opacity laws, the variation of the flux computed as a second order non-adiabatic effect would also lead to maximum luminosity at the same phase; and this certainly occurs at sufficiently great depth inside the stars. However, at the star surface, a comparison of Fig. 1a and 1c shows that maximum luminosity is delayed with respect to the radius variation, and occurs only about half-way on the ascending branch of the radius. In the same way, minimum luminosity does not occur at maximum expansion, but again about half-way on the descending branch. In other words, light maximum and minimum occur for about the same value of the radius, respectively at mid-expansion and mid-contraction. For a sinusoidal variation, this would imply a « quarter phase-lag » of the light-curve with respect to that of the radius, and the effect is usually referred to under this name. This, however, should not distract too much from the actual observations which show that, in some way, the effect is strongly bound to the general symmetry in time of the phenomenon. For instance, in  *$\delta$  Cephei* the light maximum occurs only about 0.1 of the period late with respect to the maximum contraction; while the light minimum occurs more than 0.3 of the period after

the maximum expansion. In other cepheids, like  $\zeta$  *Geminorum*, for instance, the delays are just about reversed.

The relatively small value of  $(\delta R/R)_A$ , at least for classical cepheids, might suggest that a linear theory should already provide a fairly good approximation. However one must not forget that the amplitude of other more significant physical variations, such as  $(\delta \varrho/\varrho)_A$ , may, in the external layers, become fairly large even for small values of  $(\delta R/R)_A$ . In fact, the observed variations, as illustrated in Fig. 1, exhibit in most cases an appreciable anharmonicity, which may become very strong in some *RR Lyrae* stars. This shows that non-linear effects must be at work, because even if superposition of a few linear modes could, assuming commensurability of their frequencies (which in general is very unlikely), reproduce a *periodic* phenomenon with the observed asymmetry, the maintenance of this shape will depend on the non-linear coupling between these modes. Otherwise they would, in the course of time, grow or fade away individually according to the values and the signs of their damping constants.

An interesting example, in that respect, is provided by a few stars with multiple periods studied in great details by WALRAVEN (cf. ref. A, p. 22). He has shown that, to recover the observed variation, it is necessary to superpose on the sum of the corresponding harmonic oscillations, a distortion in phase and amplitude which is a function of the instantaneous total amplitude

of that sum. Inside homogeneous groups of classical cepheids, it would also seem that the asymmetry increases with the amplitude.

This appears also clearly in a phase-plane  $(\delta R, \delta \dot{R})$ . There, the asymmetry of the phase-path suggests (cf. Fig. 2) that not only should non-linear terms be taken into account, but also that the non-conservative character of the system plays an important role in the shaping of the motion, at least in the external layers. This is also confirmed by the fact that the conservative non-

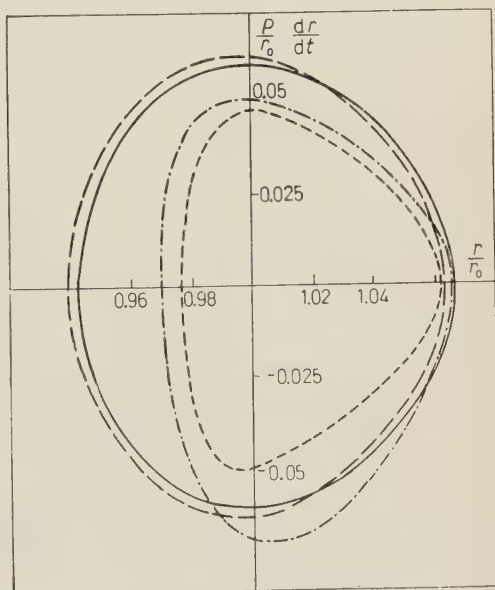


Fig. 2. - Phase diagrams for the observed pulsation of  $\delta$  *Cephei* (---) and  $\eta$  *Aquilae* (-·-·-·-). The thin lines correspond to the theoretical phase diagrams for the homogeneous model (full line) and the standard model (dashed line).

linear cases that have been discussed up to now show that the observed anharmonicity cannot be recovered for finite amplitudes of the order of those observed. Very little work has been devoted to the non-conservative case (for a more complete review cf. ref. A, chap. V) and, up to now, only the linear theory, started by EDDINGTON in 1917, has been developed to some degree of completeness.

Nevertheless, one may expect that such a theory should at least yield pe-

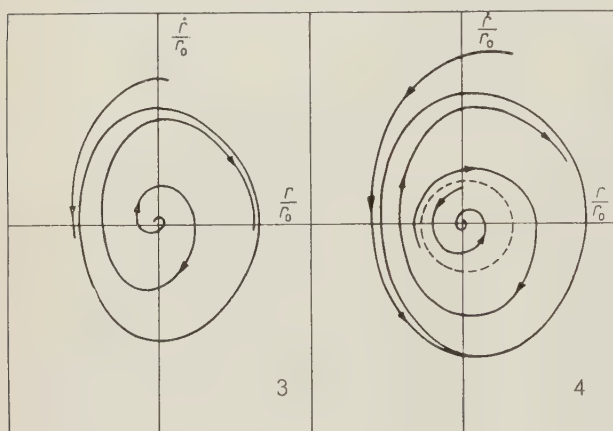


Fig. 3, 4. — Illustrations of soft self-excited oscillation (3) and hard self-excited oscillation (4). Thick full lines: stable limit-cycles; thick dashed line: unstable limit-cycle. Thin lines: phase-paths.

riods of the right order of magnitude, give some indication on the run of the amplitude inside the star, and reveal the source of the incipient instability which gives rise to the pulsation.

The last point implies that we have to deal with a *soft self-excited oscillation*; i.e., one which starts from an arbitrarily small perturbation of the equilibrium state, and increases, as illustrated on Fig. 3, along a spiralling phase-path. This path tends to a stable limit-

cycle along which, on the average, the dissipation factors balance exactly the exciting forces.

This is the point of view which has generally been adopted in this problem, although it would be difficult to advance reliable arguments ruling out definitely the case of *hard self-excited oscillations*. These, as illustrated in Fig. 4, require a finite perturbation capable of pushing the representative point of the system past the first unstable limit cycle. The general disregard of this possibility is probably due to the fact that it seems difficult, in the case of a star, to justify such a finite perturbation on a time-scale comparable to the period of pulsation. For a much slower perturbation such as we encounter normally in stellar evolution, non-adiabatic re-adjustments of the stellar structure would become dominant and could hardly lead to a pulsation such as the one considered here.

However, we have had some indications recently that some phases of stellar evolution could be extremely fast. In this connection, it may be worth-while to keep the possibility of hard self-excited oscillations in mind. Of course, in

that case the linear theory would be of no avail to elucidate the source of the pulsation.

In the absence of any adequate non-linear theory, it is obvious that we have no information on the possible limit-cycles, not even on their existence; although the latter, as we shall remark briefly later, may appear more likely for some excitation mechanisms than for others.

## 2. - Linear radial oscillations (\*).

2'1. *Development of the pulsation equations.* - In Lagrangian co-ordinates and denoting by  $\delta$  the variations of the variables following the motion, the small purely radial perturbation of a gaseous spherical star around its equilibrium configuration are governed (ref. A) by the following equations expressing:

1) Conservation of mass

$$(1) \quad \frac{\delta \varrho}{\varrho} = - \frac{1}{r^2} \frac{\partial}{\partial r} (r^2 \delta r).$$

2) Conservation of momentum

$$(2) \quad \frac{\partial^2 \delta r}{\partial t^2} = - 4 \frac{\partial r}{r} \frac{1}{\varrho} \frac{\partial p}{\partial r} - \frac{1}{\varrho} \frac{\partial \delta p}{\partial r},$$

where  $p$  denotes the pressure.

3) Conservation of thermal energy

$$(3) \quad \frac{dQ}{dt} = \frac{dU}{dt} - \frac{p}{\varrho^2} \frac{d\varrho}{dt},$$

where  $U$  represents the total internal energy

$$(4) \quad U = \frac{3}{2} \frac{RT}{\bar{\mu}} + \frac{aT^4}{\varrho} + I,$$

(\*) In view of the interest shown by aerodynamicists in the question of incipient instability underlying the pulsations of these variable stars, this part, which was intended primarily as an introduction to the problem of the external layers where the observable aerodynamic motions occur, has been amplified considerably. In writing up the final text due consideration has been paid to the numerous questions asked and the interesting comments made by many of them, especially Dr. CLAUSER, Dr. PETSCHER and Dr. THOMPSON.



with  $R$ , the gas constant,  $\bar{\mu}$ , the mean molecular weight and  $I$ , the ionization energy. We may write

$$(5) \quad \frac{dQ}{dt} = \varepsilon - \frac{1}{Qr^2} \frac{\partial}{\partial r} (r^2 F(r)) = \varepsilon - \frac{\partial L(r)}{\partial m},$$

where  $\varepsilon$  is the rate of nuclear energy generation per unit mass,  $F(r)$ , the flux per unit surface and  $L(r)$ , the total flux equal to  $4\pi r^2 F(r)$ .

Taking the first variation of eq. (3) and noting that, at equilibrium

$$(6) \quad \varepsilon = \frac{\partial L(r)}{\partial m},$$

we find

$$(7) \quad \frac{\partial \delta p}{\partial t} = \frac{\Gamma_1 p}{Q} \frac{\partial \delta Q}{\partial t} + (\Gamma_3 - 1) Q \left( \delta \varepsilon - \frac{\partial \delta L}{\partial m} \right),$$

where  $\Gamma_1$  and  $\Gamma_3$  are the generalized adiabatic coefficients relating the logarithmic variations of the pressure and the temperature to those of the density for a mixture of partly ionized gas and radiation.

The variables  $p$ ,  $Q$  and  $T$  are related by an equation of state

$$p = \frac{RQ T}{\bar{\mu}} + \frac{1}{3} a T^4,$$

and their variations by

$$(8) \quad \frac{\delta p}{p} = \beta \frac{\delta Q}{Q} - \beta \frac{\delta \bar{\mu}}{\bar{\mu}} + (4 - 3\beta) \frac{\delta T}{T},$$

where  $\beta$  represents the ratio of the gas pressure to the total pressure. The second term on the right is important only in a region where the ionization of an abundant element is critical and may vary rapidly during the pulsation and, in that case,  $\delta \bar{\mu}/\bar{\mu}$  may be expressed fairly simply in terms of  $\delta Q/Q$  and  $\delta T/T$ .

If instead of  $p$  and  $Q$ , we use  $T$  and  $Q$  as the independent thermodynamical variables we obtain instead of eq. (7)

$$(9) \quad \frac{\partial \delta T}{\partial t} = \frac{(\Gamma_3 - 1) T}{Q} \frac{\partial \delta Q}{\partial t} + \frac{1}{C_v} \left( \delta \varepsilon - \frac{\partial \delta L}{\partial m} \right),$$

where  $C_v$  is a generalized specific heat at constant volume.

The viscous terms which have been neglected here can be shown to have a very small influence on the few first modes of radial oscillation at least as

long as only molecular and radiative viscosity come into play. Furthermore, these equations are strictly valid only when radiative equilibrium prevails through the whole star. In presence of convection in some region of the star, they must be generalized and an extra equation expressing the conservation of the kinetic energy of convection must be added. As shown by COWLING, under the hypothesis of isotropic turbulent convection and adopting a mixing length picture, this generalization is not too difficult and the results have been summarized in ref. A.

The main effect is that, in such a region,  $F$  must be treated as the sum of two terms; the radiative flux

$$(10) \quad F_R = - \frac{4acT^3}{3\kappa_0} \frac{dT}{dr},$$

where  $\kappa$  is the opacity coefficient,  $a$ , the Stefan-Boltzmann constant and  $c$ , the velocity of light; and the convective flux which can be defined by

$$(11) \quad F_C = - \varrho l C C_p T \left( \frac{1}{T} \frac{dT}{dr} - \frac{\Gamma_2 - 1}{\Gamma_2} \frac{1}{p} \frac{dp}{dr} \right).$$

$C$  is of the order of a root mean square velocity of convection;  $C_p$ , a generalized specific heat at constant pressure; and  $l$ , a characteristic mixing length.  $\Gamma_2$  is related to  $\Gamma_1$  and  $\Gamma_3$  defined above.

For thermonuclear reactions, we may represent  $\varepsilon$  by

$$\varepsilon = \varepsilon_0 \varrho T^v$$

and its variations by

$$(12) \quad \frac{\delta \varepsilon}{\varepsilon} = \frac{\delta \varrho}{\varrho} + v_e \frac{\delta T}{T},$$

where however  $v_e$  may be different from  $v$  due to possible phase-delays in the variations of the abundances of the relevant elements in the course of the pulsation.

In regions in radiative equilibrium we find, according to (10) where we assume that  $\kappa = \kappa_0 \varrho^z T^{-n}$

$$(13) \quad \frac{\delta L}{L} = 4 \frac{\delta \cdot}{r} + (4 + n) \frac{\delta T}{T} - z \frac{\delta \varrho}{\varrho} + \frac{d}{dr} \left( \frac{\delta T}{T} \right) / \frac{1}{T} \frac{dT}{dr},$$

and corresponding expressions for regions in convective equilibrium can be written down using (11).

After  $\delta T$  has been expressed in terms of  $\delta \varrho$  and  $\delta p$  by means of (8) in (12)

and (13), we may substitute these expressions in eq. (7) and then proceed to the elimination of  $\delta p$  and  $\delta \varrho$  between this equation and eq. (1) and (2). However, this leads to a fifth order differential equation in  $r$ , which is very cumbersome. Since, furthermore, the last term in (7) is very small in the greatest part of the star, it has been customary to carry out the elimination of  $\delta p$  and  $\delta \varrho$  between (1), (2) and (7) without taking the explicit dependence of that last term on  $\delta \varrho$  and  $\delta p$  into account. If we separate out the time-dependence, writing

$$(14) \quad \frac{\delta r}{r} = \xi(r) \exp [i\sigma t] .$$

this leads to the usual equation

$$(15) \quad \frac{d^2 \xi}{dr^2} + \frac{d\xi}{dr} \left[ \frac{4}{r} + \frac{1}{\Gamma_1 p} \frac{d(\Gamma_1 p)}{dr} \right] + \xi \left[ \frac{\sigma^2 \varrho}{\Gamma_1 p} + \frac{1}{r \Gamma_1 p} \frac{d}{dr} \{ (3\Gamma_1 - 4)p \} \right] = \\ = \frac{1}{i\sigma r \Gamma_1 p} \frac{d}{dr} \left[ (\Gamma_3 - 1) \varrho \left( \delta \varepsilon - \frac{\partial \delta L}{\partial m} \right) \right] .$$

The solution should satisfy the following boundary conditions: at the center ( $r=0$ ):

$$(16) \quad \delta r = r\xi = 0$$

and at the surface ( $r=R$ ), treated as a free surface:

$$(17) \quad \delta p = -\Gamma_1 p \left( 3\xi + r \frac{d\xi}{dr} \right) - \frac{(\Gamma_3 - 1)}{4\pi r i \sigma} \frac{d\delta L}{dr} = 0 ,$$

since  $\varepsilon$  vanishes well below the surface.

**2'2. Adiabatic pulsation.** — Excluding a thin external layer of negligible mass, the right-hand member of (15), which represents the deviations from adiabaticity due to energy generation and conductivity, is very small. This is simply a consequence of the fact that, in a star,  $\varepsilon$  is very small compared to the internal energy per unit mass. Thus one may expect to get a good approximation for  $\sigma$  and the run of the relative amplitude  $\xi(r)$  in the bulk of the star by dropping this term completely both in (15) and (17). The latter implies that  $\xi$  and  $d\xi/dr$  remain finite everywhere, and we are left with a well defined eigenvalue problem with a discrete spectrum:  $\sigma_0, \sigma_1, \sigma_2, \dots$  associated with the complete set of orthogonal eigensolutions  $\xi_0, \xi_1, \xi_2, \dots$ .

Provided  $\Gamma_1$  does not become smaller than  $\frac{4}{3}$  in an appreciable part of the mass, all the eigenvalues are positive (no dynamical instability). The first

few modes of pulsation (corresponding to the lowest eigenvalues) have been studied numerically for many stellar models (ref. A, Table 12).

For the fundamental mode of oscillation (no node in  $0 < r \leq R$ ) one can show that  $\sigma_0^2$  is given by an expression of the form

$$(18) \quad \sigma_0^2 = C(3\bar{F}_1 - 4)\bar{\varrho},$$

where  $\bar{\varrho}$  is the mean density,  $\bar{F}_1$  an appropriate average and  $C$  depends on the actual distribution of  $\varrho$  inside the star and generally increases with the central condensation ( $\varrho_c/\bar{\varrho}$ ) of the model. Going over to the period  $P$ , (18) may be written

$$(19) \quad P_0 = \frac{Q_0}{\sqrt{3\bar{F}_1 - 4}} \sqrt{\frac{\bar{\varrho}_\odot}{\bar{\varrho}}} \text{ day}.$$

where the subscript « $\odot$ » represents values for the sun, chosen as convenient reference.

On general grounds, the fundamental mode should be the easiest one to maintain. Furthermore, the period decreases rapidly as the order of the mode increases, and this makes the comparison with observations less favorable to the higher modes. In summing up the results of this comparison, we shall thus limit ourselves to the fundamental mode.

In that case, the theoretical value of  $Q_0$  varies between about 0.07 to 0.03 as we go from the less to the most centrally condensed models that have been discussed and have a physical meaning. The most likely models for the cepheids must have a fairly high central condensation to enable the nuclear reactions to proceed at the necessary rate to explain the average luminosity. The corresponding theoretical value of  $Q_0$  may vary between 0.03 and 0.04, depending on the exact constitution of the external layers. For the classical cepheids, assuming masses obeying the usual mass-luminosity relation, the observed value of  $Q_0$  comes out around 0.032 to 0.035 so that the agreement is very reasonable in this case.

However, for cepheids of type II, the same hypothesis for the mass leads to  $(Q_0)_{\text{obs}} \simeq 0.16$ , which is quite incompatible with the theory. But evolutionary considerations suggest that the mass may be much smaller in this case; and if it is of the order of  $M_0$ ,  $(Q_0)_{\text{obs}}$  is reduced to 0.065, which is still very large. Taking into account the finite amplitudes, which are large in this case, may reduce  $(Q_0)_{\text{obs}}$  further to perhaps 0.052, which at least falls in the range of the possible theoretical values.

In the case of the *RR Lyrae* stars (at least for groups *a* and *b*) the mass has also to be taken appreciably smaller than that derived from the usual mass-luminosity relation to bring  $(Q_0)_{\text{obs}}$  in the allowed range.



For the long-period variables,  $(Q_0)_{\text{obs}}$  is also somewhat large, of the order of 0.07 to 0.06, which would not be compatible with the high degree of central condensation required by the energy generation, but here also the uncertainties on the masses are considerable.

For the  $\beta$  *Cephei* star, on the contrary,  $(Q_0)_s$  comes out rather too low, of the order of 0.022. Although masses and radii are better defined here, the readjustments necessary to bring  $(Q_0)_{\text{obs}}$  back in the theoretical range cannot be ruled out.

If one keeps in mind that for light variations associated with orbital motion or the rotation of a single convex body (assuming that the rotation axis is a symmetry axis of order 2) the smallest possible value of  $Q_0$  (contact binaries or limit of rotational stability) is of the order of 0.12, one sees that the previous discussion definitely favors pulsation.

As far as the run of the relative amplitude,  $\xi(r)$ , inside the star is concerned, it is characterized in all reasonable models and for masses of the order of those that are significant for our problem ( $M < 15 M_\odot$ ), by a large increase from the center to the surface, which takes place mainly in the external half of the model. In the «standard model», which has a very moderate central condensation ( $\varrho_c/\bar{\varrho} = 54$ ), the amplitude of the fundamental mode  $\xi_0$  increases by about a factor 2 from  $r = 0$  to  $r = R/2$ , and increases by another factor 10 from there to the surface:  $\xi_R/\xi_c \simeq 20$ . But in highly centrally condensed models ( $\varrho_c/\bar{\varrho} \simeq 10^6$ ), which are probably more significant for many of the variables considered,  $\xi_R/\xi_c$  may reach values as high as  $10^4$  to  $10^6$  depending on whether the external half is predominantly in convective or in radiative equilibrium.

This behaviour becomes more and more pronounced as one goes to higher and higher modes, the amplitude remaining fairly small up to the most external node, and increasing then very abruptly to a large value at the surface.

The behaviour of  $\delta\varrho/\varrho$  and  $\delta T/T$  is qualitatively similar except that they increase even more rapidly from the center to the surface.

**2'3. Non-adiabatic pulsation.** — We must now take into account the second member of eq. (15) and the complete boundary condition (17). Since  $p = 0$  at  $r = R$  the latter implies that

$$(20) \quad \frac{d\delta L}{dr} \rightarrow 0 \quad \text{at} \quad r = R.$$

Furthermore, as we approach the surface, keeping only the terms which increase as  $p^{-1}$  and noting that the  $\Gamma$ 's may be treated as constants there, eq. (15) becomes

$$(21) \quad \varrho \left[ -\Gamma_1 g \frac{d\xi}{dr} + \xi \left( \sigma^2 - \frac{3\Gamma_1 - 4}{r} g \right) \right] = -\frac{\Gamma_3 - 1}{i\sigma r} \frac{d}{dr} \left[ \frac{1}{4\pi r^2} \frac{d\delta L}{dr} \right].$$

where  $g$  denotes the gravity  $Gm(r)/r^2$ . As  $\varrho$  tends toward zero at  $r=R$ , we also have

$$(22) \quad \frac{d^2 \delta L}{dr^2} \rightarrow 0 \quad \text{at} \quad r = R.$$

Conditions (20) and (22) show that, in these very external layers, say above  $r=r^*$ ,  $\delta L$  may, to a high degree of approximation, be treated as a constant. Physically, this means that the pressure, the density and the heat capacity of these layers are so small that their differential motion can no longer affect the flux.

However, as we go deeper inside the star, we reach a level—say  $r=r_a$ —below which the left-hand member of (15) becomes dominant. This level can be taken as fixing the upper limit of the «adiabatic interior». It is also the level below which the last term on the right of eq. (9) becomes negligible. It occurs usually in a region where  $T$  is of the order of a few times  $10^4$  °K. We shall call the non-adiabatic region between  $r_a$  and  $r^*$  the «critical layer».

Below that layer, the right-hand member of (15) may be evaluated by means of the adiabatic solution found previously and, in that part of the star which contains practically the whole mass, it will thus be purely imaginary *i.e.* in phase with  $\pm v = d\delta r/dt$ . This means that the main correction to the adiabatic solution will be of the nature of a damping corresponding to the addition of an imaginary part to  $\sigma$ , which for the  $k$ -mode may be written

$$\sigma_k = \sigma_{k,a} + i\sigma'_k.$$

We shall neglect here any possible modification of the real part, which will be taken equal to the adiabatic frequency.

The eigensolutions will also acquire an imaginary part corresponding to a variable phase, so that the general solution will be of the form

$$(23) \quad \left(\frac{\delta r}{r}\right)_k = \exp[-\sigma'_k t] \xi_{k,a}(r) \sqrt{1 + \operatorname{tg}^2 \theta_k(r)} \cos[\sigma_{k,a} t + \theta_k(r)].$$

Very generally,  $\sigma'_k$  will be small compared to  $\sigma_{k,a}$ ; and one may evaluate it by a perturbation method, as was done first by ROSSELAND. Although due to the non-adiabatic external layers, application of the perturbation method is not quite straightforward, it yields values of  $\sigma'$  and  $\theta(r)$  which, in practice, are significant, provided that no special circumstances, such as the ionization of an abundant element, occur in the critical layer.

However, as that case is of special interest here, we shall require a more general expression for  $\sigma'$ . This can be obtained from eq. (3), which shows that to maintain one unit mass in steady pulsation, we must provide, per

period  $P$ , an amount of mechanical work

$$W_e = \int_0^P \frac{p}{Q_2} \frac{dQ}{dt} dt = - \int_0^P \frac{dQ}{dt} dt.$$

The integrands have to be evaluated up to the second order of small quantities, since the integrals of the first order terms vanish. This can be done by integrating once by parts the first integral, and then using for  $dp/dt - d\delta p/dt$  its value from eq. (7). But, if, following EDDINGTON, we remark that the change of entropy over a complete cycle must be zero, we have directly

$$\int_0^P \frac{dS}{dt} dt = \int_0^P \frac{1}{T} \frac{dQ}{dt} dt = \int_0^P \frac{1}{T} \frac{dQ}{dt} dt - \int_0^P \frac{\delta T}{T'} \frac{dQ}{dt} dt = 0,$$

or

$$\int_0^P \frac{dQ}{dt} dt = \int_0^P \frac{\delta T}{T} \frac{dQ}{dt} dt = \int_0^P \frac{\delta T}{T} \left( \delta \varepsilon - \frac{d\delta L}{dm} \right) dt,$$

using eq. (5).

From this expression of the dissipation integrated over the whole mass, it is easy to compute the damping constant for the  $k$ -mode

$$(24) \quad \sigma'_k = - \frac{1}{2\pi\sigma_{k,a} J_{k,a}} \int_0^M dm \int_0^{P_k} \left[ \frac{\delta T}{T} \left( \delta \varepsilon - \frac{d\delta L}{dm} \right) \right]_t dt,$$

with

$$J_{k,a} = \int_0^M \xi_{k,a}^2 r^2 dm.$$

In the adiabatic interior ( $m < Ma$ ), we may substitute the adiabatic solution in the integrand of the numerator and the time factor  $\cos^2(\sigma_a t)$  may be integrated out. On the other hand, since  $d\delta L/dr$  is negligible for  $m > M^*$ , and since  $\varepsilon$  certainly vanishes there, the expression (24) may be written

$$(25) \quad \sigma' = - \frac{1}{2\sigma_a^2 J_a} \int_0^{M_a} \left( \frac{\delta T}{T} \right)_a \left( \delta \varepsilon - \frac{d\delta L}{dm} \right)_a dm + \\ + \frac{1}{2\pi\sigma_a J} \int_{M_a}^M dm \int_0^{2\pi/\sigma_a} \left[ \left( \frac{\delta T}{T'} \right)_a + \left( \frac{\delta T}{T} \right)_{na} \right] \frac{d\delta L}{dm} dt,$$

where we have dropped the index  $k$  as, from now on, we shall be mainly interested in the fundamental mode. This expression is also known as the coefficient of vibrational stability, and the star is said to be vibrationally stable or unstable (overstable) depending on whether it is positive or negative.

Let us first discuss the first term on the right-hand side of (25) which, in some cases, is the only one that matters. Expression (12) shows that  $\delta\epsilon$  is always of the same sign as  $\delta T$ , and the energy generation contributes negatively to  $\sigma'$ ; i.e. it always tends to increase the amplitude or, in other words, to render the star vibrationally unstable.

As to the term in  $\delta L$ , an integration by parts using eq. (13) and remembering that  $\delta L$  vanishes at the center, gives

$$(26) \quad \int_0^{M_a} \left( \frac{\delta T}{T} \right)_a \left( \frac{d\delta L}{dm} \right)_a dm = \left\{ L \left( \frac{\delta T}{T} \right)_a \left[ 4\xi + (4+n) \frac{\delta T}{T} - \chi \frac{\delta \rho}{\rho} + \frac{(d/dr)(\delta T/T)}{(1/T)(dT/dr)} \right] \right\}_a - \int_0^{M_a} \left[ 4\xi + (4+n) \frac{\delta T}{T} - \chi \frac{\delta \rho}{\rho} + \frac{d}{dr} \left( \frac{\delta T}{T} \right) \right] \frac{1}{T} \frac{dT}{dr} \frac{d}{dm} \left( \frac{\delta T}{T} \right)_a dm.$$

In the bracket of the integrated part, the first, third ( $\chi \neq 0$ ) and fourth terms are always of the opposite sign to that of  $\delta T$ ; thus they contribute negatively to  $\sigma'$  and reinforce the instability. Physically, they correspond to the different factors that tend to decrease the flux at contraction: the decrease of the radiating surface, the increase of the opacity associated with its proportionality to  $\rho$ , and the decrease of the temperature gradient; and vice-versa, at expansion. As the energy generation, these factors tend to heat up the gas at compression and to cool it at expansion.

On the other hand, the second term in the bracket with ( $n > 0$ ) has the same sign as  $\delta T$ , and gives a positive damping which contributes to the stability of the star. Physically it corresponds to the increased radiation power per unit surface at compression, and the decrease of the opacity associated with its dependence on a negative power of  $T$ .

As an illustration, let us assume that the first and fourth terms in the bracket amount together to about  $-\delta\rho/\rho$ , then eliminating  $\delta T/T$  by means of the adiabatic part of the relation (9), the whole bracket may be written

$$(27) \quad [(4+n)(\Gamma_3-1) - (\chi+1)] \frac{\delta\rho}{\rho}.$$

For the usual Kramers opacity law ( $\chi=1$ ,  $n=3.5$ ) and a star of fairly small mass ( $\Gamma_3 > \frac{5}{3}$ ), the first term predominates and all together, the integrated term in (26) will have a strongly stabilizing influence. In practice, numerical



computations confirm that the same situation prevails for all reasonable opacity laws provided the mass does not become very large.

The same type of reasoning may be applied to the integral in the second member of (26), the different terms having now the opposite effect because of the negative sign before the integral. Thus, on the whole, in the same conditions as above, this term has a destabilizing influence. However, due to the rapid increase of  $\xi_a$ ,  $(\delta T/T)_a$  and  $(\delta \varrho/\varrho)_a$  from the center outwards, the integrated term will be largely preponderant; and more so for higher modes since, in that case, parts of the integral on successive regions will tend to cancel out. Thus, on the whole the « conduction » will tend to damp out the oscillation.

We must now compare this stabilizing effect to the destabilizing influence of the energy generation. The latter is limited to a very small central core where all the amplitudes are small while, as we have just seen, the external layers where the amplitudes are large are determinant for the « conduction » effect. Since these opposing influences are proportional to the squares of the amplitudes, it is understandable that, in all cases satisfying our general hypothesis (fairly small masses, small heat capacity of the non adiabatic layer) the coefficient of vibrational stability comes out positive and large. For a not unreasonable model of a cepheid, Cox (1955) found that the destabilizing influence of  $\epsilon$  would lead to an increase of the amplitude by a factor  $e$  in an extremely long time, of the order of  $10^9$  years, while the damping time due to « conduction » is only of the order of 10 days. It is obvious that, in such a case, only a strong reversal of the stabilizing trend in the external layers could bring about vibrational instability.

As we go to larger and larger masses, the pressure of radiation becomes an increasingly large fraction of the total pressure and  $\Gamma_1$ ,  $\Gamma_2$  and  $\Gamma_3$  decrease and tend towards  $\frac{4}{3}$ . This has two effects: it reduces considerably the rise of the amplitude with  $r$  (for  $\Gamma \rightarrow \frac{4}{3}$ ,  $\sigma_0 \rightarrow 0$ ,  $\xi \rightarrow C^i$ ) so that the energy generation is no longer in such an unfavorable position with respect to the « conduction ». Furthermore, the stabilizing effect of the latter decreases, as can be seen from (26) when smaller and smaller values of  $\Gamma_3$  are used. The result is that, for any law of thermonuclear energy generation, there is always a critical mass above which the stars become strongly vibrationally unstable (LEDoux, 1941; SCHWARZSCHILD and HÄRM, 1959). Unfortunately, this occurs at much too high masses to be of interest for our problem; furthermore, the instability becomes so strong for small excesses of the mass above the critical value, that it seems likely to lead to strong ejection of material rather than to a regular pulsation.

In all this, we have assumed radiative equilibrium. But the presence of *limited zones* in convective equilibrium does not alter the main conclusion since the convective transfer of energy either increases at contraction and de-

creases at expansion, thus having a stabilizing influence just as radiative conductivity (time of relaxation of convection short as compared to the period) or does not vary appreciably its effects on stability being very small (time of relaxation long).

Thus, up to now, we haven't discovered any source of vibrational instability of significance for the regular variable stars. However, as illustrated above for massive stars, a lowering of the  $\Gamma$ 's—whatever its origin—would favor instability. Long ago, EDDINGTON noted that this could also be brought about by the ionization of an abundant element, at least in limited regions of the star. However, these regions will be so small that they will practically not affect  $\sigma$  or the run of  $\xi$ ; so that the main effect described above for massive stars will be absent in this case. For this reason, ionization of heavy elements occurring deep in the star will not be of much help and, anyway, with the large preponderance of H and He (98 to 99 percent of the mass) now accepted, such ionization would not even modify the  $\Gamma$ 's appreciably. This led EDDINGTON (1942) to propose a qualitative theory in which the instability responsible for the development of pulsation in cepheids was attributed to the ionization of hydrogen. However, this occurs in the critical layer  $r_a$  to  $r^*$ , where the non-adiabatic terms are very important, and it is essential to take them into account. Later quantitative discussions have failed to provide definite support for Eddington's suggestion, but they have led to some clarification of the important physical factors and to the development by ZHEVAKIN (1960) of a modified version in which the second ionization of helium now plays the fundamental role. As this occurs already deeper in the star, the non-adiabatic aspects of the phenomenon, although still significant, are perhaps not quite as essential as in Eddington's original theory.

Let us note that, due to the large separation of the ionization potentials of H and He, the corresponding ionizations occur in fairly distinct regions. In the middle of such a region (50 percent ionization), the  $\Gamma$ 's reach fairly low values depending on the abundance of the element and its ionization potential. But even with a He-abundance by number of about 15 percent, the values of the  $\Gamma$ 's associated with its second ionization may be as low as 1.25, according to ZHEVAKIN. In all cases, the mass of the corresponding region is too small for this local change in compressibility to affect appreciably the period of the pulsation or even the local amplitude of the displacement. However the adiabatic variation of the temperature (first term on the right-hand side of (9)) will be much reduced there, and this alone can change the sign of the quantity in brackets in (26) or (27). But it is also found (SCHATZMAN (1956), ZHEVAKIN (1960)) that  $n$  decreases considerably below the usual values ( $\sim 3$ ), and may even become negative in such a region—especially on the external side of it—and this enhances very much the reversal in the effect of the radiative conductivity. Furthermore, taking for instance the situation at compression,

the gradient of the modified temperature will be steeper on the internal side of the ionization zone, and less steep on the external side, favoring the inflow of energy in it and hindering its escape (influence of the last term in the bracket), thus also reinforcing the instability.

Of course, we shall have, superposed on those effects, those of non-adiabaticity, a measure of which is provided in a first approximation by

$$(28) \quad q = \frac{[\delta L(r_a) - \delta L(r^*)]_a P}{\pi(M^* - M_a) C_v(\delta T)_a},$$

which is the ratio of the heat accumulating in this layer due to the quasi-adiabatic effects discussed above during a quarter-cycle, to the variation of its heat content due to compressional work during the same interval.

To discuss rigorously these non-adiabatic effects, we would have to continue, for  $r > r_a$ , the adiabatic solution by the complete complex solution of eq. (25) after  $\delta \varrho$  and  $\delta T$  have been expressed in terms of  $\xi$  in  $\delta L$  ( $\delta \varepsilon = 0$ , here). As mentioned under eq. (13), this leads to a complicated high order differential problem. Furthermore, the variable  $\xi$  is probably not particularly appropriate because as shown by EDDINGTON and confirmed by ZHEVAKIN (1960), it is affected very little by the non-adiabatic terms, its imaginary part (or its phase-shift across the region, cf. (23)) remaining very small. As a consequence,  $\delta p$  will also remain very close to the adiabatic solution so that, from (7) and (9), the non-adiabatic component of  $\delta T$  will satisfy the following approximate equation

$$(29) \quad \frac{\partial}{\partial t} \left( \frac{\delta T}{T} \right)_{\text{n.a.}} = - \frac{1}{\Gamma_1 C_v T} \left[ \Gamma_1 - \frac{(\Gamma_3 - 1)^2 \beta C_r}{(\gamma - 1) c_v} \right] \frac{\partial \delta L}{\partial m} \simeq - \frac{1}{\Gamma_1 C_v T} \frac{\partial \delta L}{\partial m}.$$

In this respect, one may verify from the continuity equation that a very small non-adiabatic component  $\xi_{\text{n.a.}}$  is sufficient to cause a fairly large readjustment of  $(\delta \varrho / \varrho)$ , capable of compensating  $(\delta T / T)_{\text{n.a.}}$  without any appreciable change in  $\delta p / p$ .

If we assume with EDDINGTON that the dominating term on the right of (13) is the one in  $\delta T / T$ , and that, in the critical layer ( $r_a - r^*$ ), the non-adiabatic part of  $\delta T / T$  is largest, eq. (29) becomes

$$(30) \quad \frac{\partial}{\partial t} \left( \frac{\delta L}{L} \right) \simeq - \frac{L(4 + n)}{\Gamma_1 C_v T} \frac{\partial}{\partial m} \left( \frac{\delta L}{L} \right),$$

which admits a solution

$$(31) \quad \frac{\delta L}{L} = \left( \frac{\delta L}{L} \right)_{M_a} \cos [\sigma_a t + \varphi(m)],$$

with a phase-lag

$$(32) \quad \varphi(m) = -\frac{2\pi \int_{M_a}^m C_v T dm}{(4+n)LP},$$

defined essentially as the ratio of the heat content of the layer to the total energy radiated across it in one period. This confirms our previous statement that the layers with very small heat capacity cannot affect the flux.

Let us go back now to the discussion of the vibrational stability. Taking (29) into account, the time integral involving  $(\delta T/T)_{n,a}$  in the last term of (25) cancels out; and using (31), the expression (25) of  $\sigma'$  may be written

$$(33) \quad (2\sigma_a^2 J_a) \sigma' = - \int_0^{M_a} \left( \frac{\delta T}{T} \right)_a \delta \varepsilon_a dm - \int_0^{M_a} (\delta L)_a \frac{d}{dm} \left( \frac{\delta T}{T} \right)_a dm + \\ + \left[ \left( \frac{\delta T}{T} \right)_a \right]_{M^*} (\delta L)_{M_a} \cos [q(M^*)] - \int_{M_a}^{M^*} (\delta L)_{M_a} \cos [q(m)] \frac{d}{dm} \left( \frac{\delta T}{T} \right)_a dm.$$

As in our discussion of the quasi-adiabatic part of (25) or of (26), the first two terms in (30) contribute to the instability while the integrated stabilizing term in (26) is replaced here by the third term proportional to  $\cos[q(M^*)]$ . As long as  $|q(M^*)| < \pi/2$ , it is still positive and reinforces the stability.

But as we increase the heat capacity (or  $C_v$ ) of the layer ( $M^* - M_a$ ) (and the ionization of an abundant element is the only way to achieve this),  $|q(M^*)|$  also increases; and the contribution of the third term to the stability decreases until for  $|q(M^*)| = \pi/2$ , it vanishes altogether. In that case, the last term has a small stabilizing influence [ $d(\delta T/T)/dm < 0$  in the region where  $q(m)$  is small] which may at most balance the second term in (33). If  $|q(M^*)|$  increases above  $\pi/2$ , the third term now reinforces the instability, but the stabilizing contribution of the last term increases too [ $d(\delta T/T)/dm > 0$  towards the top of the critical region where  $\cos q(m) < 0$ ] and EDDINGTON thought that this indicated that the minimum dissipation (or maximum instability) would occur for  $q(M^*)$  in the vicinity of  $\pi/2$ . As one may well admit that pulsation occurs only when this condition of maximum instability is realized, this would, at the same time, explain the observed phase-lag between displacement and the flux in cepheids. Furthermore it would open the way to an explanation of their period-luminosity relation (or of their distribution in a narrow band in the H-R diagram) since, apart from the mass-luminosity relation and the period-mass-radius relation provided by (19), this condition of maximum instability (or  $q(M^*) = \pi/2$ ) would furnish a third relation between  $L$ ,  $M$  and  $R$  and thus permit the elimination between the three equa-



tions of two of the parameters say  $M$  and  $R$  (or, in the H-R diagram, of  $M$  and  $P$ ).

However, we may note immediately that in Eddington's scheme, the final source of instability is still the first term in (33) due to the energy generation; and we have seen already that, for likely models of supergiants like the cepheids, this yields a much too small rate of increase of the amplitude. Thus what we really want to find is not just the cancellation of the dissipation but a negative value for it capable of bringing any small pulsation up to an appreciable amplitude in a reasonable time.

First, we could try to verify whether Eddington's views on maximum instability occurring for  $q(M^*) \simeq \pi/2$  are correct. It seems that when  $q(M^*)$  becomes greater than  $\pi/2$ , the destabilizing influence of the integrated term in (33) may very well dominate any possible stabilizing effects of the last term; and it may be worth-while to investigate this point, even if it means losing the right phase-lag in this approximation.

However, Eddington's approach suffers from other difficulties. Even in the ionization zone of hydrogen (nearest to the surface and strongest non-adiabatic effects), it is not at all certain that the non-adiabatic terms are so dominant as to justify the corresponding hypothesis in the establishment of (30). Furthermore, this equation rests also on the assumption that the term in  $\delta T/T$  is largest in eq. (13). But in the ionization zone,  $I_3$  and  $n$  tend to become so small that, at least in the quasi-adiabatic approximation, this term is far from dominant. In particular, the term proportional to the gradient of  $\delta T$  may become large close to the extremities of the critical layer.

We then suspect that the variation of  $(\delta L/L)$  in the critical layer may indeed be very different from the simple phase-shift given by (31), and that it is going to be necessary to solve the non-adiabatic equation much more carefully. Furthermore, convection tends to get established in those layers of low  $I$ 's, and if it takes an appreciable part in the energy transport, it will certainly decrease the instability.

Although no detailed discussions of the non-adiabatic effects of the ionization zone of hydrogen exist, one is tempted to conclude from Schatzman's quasi-adiabatic treatment (1956), when corrected by the introduction of an appropriate limit  $M^*$  to the integrals in  $\sigma$  (cf. ref. A, Section 69), that it occurs too far out to provide the necessary instability.

This favors Zhevakin's point of view that the source of instability should be looked for deeper in the star, in the region of the second helium ionization. According to him, although convection arises in this layer, the superadiabatic excess of the radiative gradient remains so small that the flux is still mainly transported by radiation, so that  $\delta L$  is still given by (13). In that case, ZHEVAKIN finds that vibrational instability prevails for a whole range of values of  $q$ , as given by (28), corresponding to all possible phase-lags from practically

0 to  $180^\circ$  depending on the ratio of the mass of the critical layer to the mass above it, a circumstance in which he sees the possibility of explaining practically every type of observed stellar variability. However, no account has been taken of the first ionization zone of helium, or of that of hydrogen which, despite the fact that they may not suffice to cause instability, may still affect the phase-lag appreciably. One should note also that, from his latest computations (ZHEVAKIN, 1960), it appears that already a large fraction of the total negative dissipation arises below what he calls the critical layer of the second helium ionization.

In general, although ZHEVAKIN has devised an ingenious algebraic method based on the subdivision of the star into discrete layers to solve his non-adiabatic equations in the external zone, his models and the application of his method itself (for instance the whole critical layer is one of the discrete shells) are still very rough, so that the results fail to be completely convincing.

COX (1958) has also discussed the problem of the non-adiabatic layers, trying to formalize some of its aspects; for instance, by assimilating the effect of the critical ionization to a sudden and large drop in  $\delta L$ , so as to permit a simple mathematical treatment in terms of the parameter  $q(M^*)$  as defined by (32). Later COX and WHITNEY (1958) and COX (1959), using this formalism, found that, if the region of the second helium ionization is responsible for the sudden change in  $\delta L$ , then the condition  $q(M^*) = \pi/2$ , used as a criterion for maximum instability, is compatible with the observed period-luminosity relations for cepheids of types I and II.

Since then, new investigations by COX (1960), which provide certainly the best treatment of the non-adiabatic layers ever attempted, have confirmed the destabilizing effect of the second helium ionization zone for reasonable abundances of helium (15 percent, by number) assuming that radiative transfer is dominant there. However, the negative dissipation in that region is of the same order as the estimated positive dissipation in the interior so that his results are somewhat unconvincing as far as the overall vibrational instability of the star is concerned. Nevertheless, it may be noted that the condition of minimum total dissipation in the star (this time without any assumption as to the corresponding phase-lag  $q(M^*)$ ) does again lead to a fairly satisfactory period-luminosity relation for the Population I cepheids. Due to the low surface gravities of the type II cepheids, this mechanism does not seem to work in their case.

Let us add that all these computations have been made for very idealized models of the external envelopes and neglect the effects of the first helium and hydrogen ionization. While the consequences of the first point are difficult to foresee, it may be reasonable to expect that, if taken properly into account, the second may improve some of the results and increase somewhat the negative dissipation.

Apart from the complexity of the rigorous non-adiabatic equations, one major difficulty resides in the fact that we do not have, at present, any reliable model for the interior of these variables and this may affect considerably the gradient of the different perturbations  $\delta r$ ,  $\delta \rho$ ,  $\delta T$  in the external layers.

Nevertheless, the results reported above show definitely that the ionization of an abundant element in the external non-adiabatic layers, with the corresponding changes in the values of the  $I$ 's, of the opacity and of the temperature gradients, may lead to the accumulation of heat at the expense of the radiative flux in that layer during the phases of compression, and thus produce instability if the heat capacity of the layer including the displacement of ionization equilibrium is large enough.

A general advantage of negative dissipation as a source of the vibrational instability responsible for the pulsation is that its effects have a natural limit. In our case, once the amplitude is large enough so that, at compression, practically all the atoms primitively in a critical stage of ionization are completely ionized, the source of the instability vanishes and positive dissipation takes over, limiting the amplitude to some finite value.

However, a word of caution may be in order. All the previous discussions were supposed to refer to the fundamental mode of oscillation. But the behavior of the first mode or even the second is not so different in the external layers from that of the fundamental mode; and the arguments, as far as they have been developed, would apply just as well to any of these modes. The viscous dissipation is not very much higher either for these first few modes (COUNSON, LEDOUX, SIMON, 1956) and, once excited, it will be difficult to get rid of them, although no traces of such modes are found in most of the observed light or velocity curves. This seems to be one of the fundamental difficulties associated with pushing the source of the instability far out into the external layers.

### 3. - The atmospheric problem.

All the observations refer directly to the atmospheric layers, and it is obvious that our views on the general phenomenon depend strictly on a correct interpretation of these observations. However, if one admits that the observed radial velocity fields and light-variations are conclusive evidence of pulsations, the preceding sections may lead him to think that the atmosphere plays a negligible role in the problem since its mass and its heat capacity are so small that it can neither affect the period  $P = 2\pi/\sigma$  (cf. eq. 18) nor the coefficient of vibrational stability  $\sigma'$  (cf. eq. 25).

But this is not quite true, because the boundary conditions depend very much on what we believe is the structure of the most external layers. In the preceding discussion, we have always assumed that the pressure vanishes at



some point,  $r = R$ , insuring perfect reflection there. If this is correct, the oscillations, at least in the linear approximation, should have essentially the character of a standing wave right through to the surface except for the very small phase-shift  $\theta(r)$  due to non-adiabaticity (cf. eq. 23).

If on the other hand the star had no such sharp boundary but would fade out gradually into the surrounding interstellar medium, the wave could take a marked progressive character in the external layers and an appreciable part of the wave energy would be lost to that medium, increasing appreciably the damping (cf. ref. A, section 68; SCHWARZSCHILD and HÄRM, 1959). Not much is known of the exact circumstances under which this would occur. If we assume that the star is surrounded by an extensive hot corona with a much lower density than the atmosphere proper, an appreciable fraction of the energy could still, even at the wavelengths considered here, filter through the density discontinuity separating the atmosphere from the corona.

Furthermore, in the absence of a proper boundary, the amplitude would in the linear approximation increase indefinitely, leading unavoidably to a breakdown of this approximation, to the formation of shock-waves and to non-linear damping effects. Of course, for finite amplitudes, this type of effect may already occur in the atmosphere proper, even in the presence of a sharp boundary, and may indeed be one of the mechanisms which, in the presence of vibrational instability, stops the increase of the amplitude and stabilizes it at some finite value.

There are two approaches to this aspect of the problem. One may, for instance, build purely theoretical models, try to solve the corresponding aerodynamical problems completely and compare the solutions with the observations until some kind of fit is found. However, at the present time, this is likely to lead to a great many useless trials in all kinds of directions. Instead we may start directly from the observations, which we analyse in as much detail as possible and, if necessary, we make new observations to find out what is the actual behaviour of the external layers in the course of the pulsation. In particular, we try first to answer the two following questions, which are not quite independent:

1) Does the atmosphere, at any instant, deviate appreciably from the normal atmosphere of a non pulsating star, and what are these deviations? Or can we, at each phase, explain its properties by the usual theory of static atmospheres?

2) What is the character of the wave in the external layers (standing, progressive, shock)?

Our two sources of information are the distribution of the radiation in the continuum and the line-spectrum.



3'1. *The continuum.* — The study of the continuum has improved continuously with the development of better and better techniques. At first it reduced essentially to the determination of a color index or at most a few color indices. This type of study culminated in the six color photoelectric photometry of STEBBINS and collaborators (STEBBINS, 1953).

Spectrophotometry, which consists in measuring the intensity of the continuous spectrum between the absorption lines at as many wave-lengths as possible, provides, in principle, an ideal method. However, as long as its application had to be made photographically, it presented in practice many limitations, and required an enormous labor in transforming from photographic density into true intensities. But direct photoelectric scanning with high resolution of the spectrum has now become possible, achieving a considerable gain in precision and speed, and we shall describe later some of the results obtained by J. B. OKE using this method.

If we denote by an asterisk, values relative to some standard star, the « relative gradient »

$$\Delta\Phi = -2.3 \frac{\Delta \log (I_\lambda/I_\lambda^*)}{\Delta(1/\lambda)},$$

is fairly constant in rather large intervals of  $\lambda$  and can be used to characterize the distribution of energy in at least a part of the spectrum. If the stars were radiating like black-bodies, we would have

$$\Delta\Phi = \frac{C_2}{T'} (1 - \exp[-C_2/\lambda T'])^{-1} - \frac{C_2}{T^*} (1 - \exp[-C_2/\lambda T^*])^{-1},$$

with  $C_2 = 14300$  if  $\lambda$  is expressed in Å and  $T$  in °K. In the range of temperatures, considered, we may write

$$\Delta\Phi \simeq \frac{C_2}{T'} - \frac{C_2}{T^*},$$

which defines the color temperature of the star and is very closely related to the color index C.I. =  $m_{\lambda_2} - m_{\lambda_1}$ , if  $\lambda_2$  and  $\lambda_1$  lie in the same range of wave-lengths.

Around 1940, BECKER and STROHMEIER found that they could represent relatively well the distribution of intensity in cepheids at any phase by means of two gradients, corresponding respectively to the following ranges of  $\lambda$ :  $(6500 \div 4800)$  Å and  $(4800 \div 3900)$  Å. They derived corresponding color temperatures and their variations in the course of the pulsation.

These temperatures however presented considerable differences with the « radiation temperatures » defining the surface brightness of the star. As a

rule, the color temperatures are larger at maximum light and smaller at minimum light than the corresponding radiation temperatures. Thus, the amplitude of the color temperature is much larger than that of the radiation temperature. This departure from black-body radiation points to a real difficulty in the straightforward interpretation of the variations in light and color in terms of the variation of the radius and temperature.

CANAVAGGIA (1949), also determined two gradients in the continuous spectrum: before (4800 to 4000 Å) and after (3600 to 3100 Å) the Balmer discontinuity for  $\delta$  Cephei,  $\eta$  Aquilae and  $\xi$  Geminorum. She also evaluated the Balmer discontinuity  $D = \log(I_{3700+}/I_{3700-})$  which varies more or less in parallel to the brightness of the star. These variations affect strongly the gradient in the violet, so that the color temperature derived from it has no physical meaning. On the other hand,  $D$  is very sensitive to the value of the gravity and can be used to determine its effective value,  $g_e$ , at different phases.

In principle, six-color photometry (STEBBINS used  $\lambda\lambda$  10300(I), 7190(R), 5700(G), 4880(B), 4220(V) and 3500(U)) yields a much larger amount of information on the continuous spectrum than the determination of one or two gradients since the differences between any two of the six corresponding curves may be used as so many measures of the variations in color of the star during the pulsation. However, one must keep in mind that the band-widths of the filters are quite considerable (a few hundreds Å) so that the influence of the absorption lines is not eliminated. In particular, the amplitude of the U-curves is still strongly affected by the Balmer discontinuity and, in many investigations, it cannot be used.

Furthermore, these measures are affected by the general interstellar reddening, an effect which was neglected in the earlier attempts at interpretation (HARRIS, 1954).

One of the first effects established by the six-color observations is the phase-shift between the light-curves in different colors; the maximum and the minimum, which remain equally distant, occurring progressively later as one goes from the U-curve to the I-curve, the phase retardation for this last curve amounting to about  $0.05 P$ . As predicted by VAN HOOFF (1943), this is really what one should expect on the simple picture of a star radiating like a black-body due to the fact that in a cepheid, according to the usual interpretation of the velocity curve,  $R$  increases while  $T_e$  goes through its maximum. This simple approach should be corrected for the black-body deviations; but WESSELINCK (1947) has shown that, qualitatively, the effect remains the same.

Even if exact quantitative agreement has not been reached, this effect and its qualitative interpretation certainly support the pulsation theory and the ordinary interpretation of the radial velocity curve as being practically identical to that of the photosphere.

But the six-color observations can yield other tests of the pulsation theory, which are all related to a very simple criterion first proposed by BAADE (1926): if a cepheid radiates like a black-body, at all phases,

$$(34) \quad L(t) = 4\pi R^2(t)\sigma T_e^4(t)$$

and if  $T_e(t)$  is known throughout the cycle as a function of the phase (from spectral type or color-index), the relative variation of the radius  $R(t)/R_0$  may be computed from (34) and should agree in phase and amplitude with the displacement  $(R(t) - R_0)$  derived from the radial velocity curve for a value of  $R_0$  compatible with the average luminosity  $L$  and the average effective temperature  $(T_e)_0$  of the star. Of course, Baade's criterion may be formulated in terms of monochromatic luminosities as well.

The difficulties encountered in the first application (BOTTLINGER, 1928) were attributed to deviations from the black-body laws, which were confirmed by BECKER's investigations (1940). Then, the main step in the application of Baade's test is to find a correlation between the color-index  $\text{C.I.} = m_{\lambda_2} - m_{\lambda_1}$  and the surface brightness  $b_{\lambda_1}$  expressed in magnitudes in a small interval around  $\lambda_1$ , say

$$(35) \quad b_{\lambda_1} = f(\text{C.I.}) .$$

In the absence of a detailed theory of stellar atmospheres, such a relation could only have an empirical basis. BECKER himself had already obtained one by comparing  $\Delta\text{C.I.}$ 's and the  $\Delta m_{\lambda_1}$ 's between the phases of maximum and minimum in a series of cepheids *admitting that, at these phases, the radii are equal* so that the  $\Delta m_{\lambda_1}$ 's reduce to the  $\Delta b_{\lambda_1}$ 's. Of course, this assumption is a consequence of the usual interpretation of the radial velocity curve. It implies already to a certain extent that the pulsation theory is correct; and that the variation of the photospheric radius,  $R_p$ , is parallel to that of the radius,  $R_v$ , characteristic of the level where the absorption lines are formed. With his relation (35) BECKER found that Baade's test was, on the average, well satisfied but the overall precision was not very high.

VAN HOOFF (1943) later extended Becker's method to all pairs of phases in a given cepheid which, according to the velocity curve, have the same radius. This is sufficient to establish a relation of type (35), which can usually be written

$$(36) \quad \Delta b_{\lambda_1} = a_{\lambda_1} \Delta(\text{C.I.}) .$$

If the variation of C.I. is known through the cycle, the variation of  $(R_p)_0$

between any two phases 1 and 2 can be obtained from

$$(37) \quad (\Delta m_{\lambda_i})_{1,2} = -2.151 \frac{(\Delta R_p)_{1,2}}{(R_p)_0} + a_{\lambda_i} (\Delta \text{C.I.})_{1,2},$$

where  $(R_p)_0$  is the photospheric radius at some phase, say the mean radius. Identifying  $(\Delta R_p)_{1,2}$  given by (37) to the corresponding displacement  $(\Delta R_r)_{1,2}$ , obtained by integration of the velocity curve, one can determine a value of  $(R_p)_0$  for each pair of phases (1, 2) considered. Since the method is rather sensitive to small errors in the empirically determined values of  $(\Delta m_{\lambda_i})_{1,2}$  and  $(\Delta \text{C.I.})_{1,2}$ , an agreement in order of magnitude between the different values of  $(R_p)_0$  is generally considered a satisfactory test.

WESSELINCK (1946) took a new step in considering phases of equal C.I., freeing himself of relations of the type (36), although he still assumes that  $\Delta b_{\lambda_i}$  vanishes with  $\Delta(\text{C.I.})$ . Now, if 1 and 2 denote phases corresponding to the same C.I., eq. (37) yields

$$(\Delta m_{\lambda_i})_{1,2} = -2.151 \frac{(\Delta R_p)_{1,2}}{(R_p)_0}.$$

As before a set of  $(R_p)_0$  can be derived, which should be consistent.

The six-color photometry gave a new impetus to these methods: in particular the corresponding phases in Wesselinck's method could be determined by a much better match in colors (WESSELINCK 1947, STEBBINS, KRON and SMITH 1952). On the whole, in the case of the cepheids, the values of the mean radius thus obtained show a reasonable agreement with other determinations.

A combination of Van Hoof's and Wesselinck's methods, with an empirical determination of the  $a_{\lambda_i}$ 's in the different relations (36) corresponding to Stebbins' six color observations, can lead also to the absolute values of the radius as a function of phase (OPOLSKI and KRANĚCKA 1956).

However only a theoretical discussion can reveal the complete meaning of these tests. The simplest approach is to assume that, at each phase, the atmosphere adjusts itself practically instantaneously to the radiative flux coming from the interior and to the effective gravity  $g_{\text{eff}}$ :

$$(38) \quad g_{\text{eff}} = \frac{GM}{R^2} + \ddot{R},$$

where  $R$  and  $\ddot{R}$  are the instantaneous values of the radius and the acceleration, which is supposed uniform throughout the atmosphere.

One may then build a series of static model atmospheres for an appropriate range of values of  $T_e$  and  $g_e$ . Each of these models gives the flux,  $F_{\lambda}$ , as a



function of frequency and a value for the Balmer discontinuity  $D$ , which is rather sensitive to  $g_{\text{eff}}$ . It remains then to pick out the models that reproduce at each phase the observed six-color magnitudes and  $D$ . Then the relative variation of the photospheric radius may be computed from

$$(39) \quad (m_\lambda)_i - (m_\lambda)_0 = 2.5 \log \left( \frac{F_{\lambda,0}}{F_{\lambda,i}} \right) - 5 \log \left( \frac{R_{p,i}}{R_{p,0}} \right),$$

where the left-hand member represents the observed magnitude difference at the wavelength  $\lambda$  between any pair of phases  $i$  and 0 and the  $F_\lambda$ 's, the theoretical flux given by the appropriate models.

This can be repeated for a series of colors  $\lambda$ , providing an internal test on the values of  $R_p/R_{p,0}$ . As a rule, the observations in the ultraviolet are not used because they are strongly affected by the Balmer discontinuity. Moreover, the theoretical  $F_\lambda$ 's should really refer, not to a given wavelength but to the total flux passing through each of Stebbins' filters.

The relative variations of the radius  $R_p/R_{p,0}$  thus determined should agree in phase with those of  $[R_p - (R_p)_0]$ . The agreement in amplitude permits one to determine the mean radius  $R_{p,0}$ . Furthermore, the values of the  $g_{\text{eff}}$ 's fixed by the fitting of the models to the observations (especially  $D$ ) should agree with those which can be computed from (38), provided a likely value of the mass is known.

The first attempts (HITOTUYANAGI, 1952; CANAVAGGIA and PECKER, 1952*a*, 1952*b*, 1953; LEDOUX and GRANDJEAN, 1954) to carry this test through encountered considerable difficulties. The method is essentially equivalent to a theoretical determination of the  $a_i$ 's in (36) for the different wavelengths used; and the models, apparently like the black-body, yielded too large values for these.

However, further corrections to the observations were necessary due to the effect on the continuum of the variations in intensity of the absorption lines with phase and to the interstellar reddening (HARRIS, 1954; CANAVAGGIA, 1954, 1955). As shown by WHITNEY (1955), the first factor acts in the right direction to improve the agreement between  $R_p$  and  $R_r$ , and this was partially confirmed by HITOTUYANAGI and VIJ-IYE (1956) in an investigation based on photographic spectrophotometric data.

Recently J. B. OKE (1960) has considered the problem again for *RR Lyrae* and  *$\eta$  Aquilae*, using photoelectric scanning of the spectrum, and he has kindly sent a detailed summary of his results in advance of publication. In the case of *RR Lyrae*, the observations were made at the Cassegrain focus of the 100 inch telescope and the resolution of the scan extending from 6000 to 3300 Å is approximately 5 Å in wavelength and 15 min in time.

HD 182487, whose energy distribution had been calibrated on an absolute

scale by comparison with Vega using Code's results, was used as a standard; and monochromatic light curves of the observed differences between *RR Lyrae* and HD 182487, outside the atmosphere, were plotted for 24 wavelengths. Corrections for absorption lines were carefully computed from intensity tracings of Sanford's 10 Å/mm spectrograms. No corrections for interstellar reddening were applied since, according to STRÖMGREN, it is very small in the *RR Lyrae* region. By adding to the corrected measurements the absolute energy distribution of HD 182487, the absolute energy distribution in the spectrum of *RR Lyrae* was obtained at each phase.

By comparison with the absolute flux of model atmospheres computed by DE JAGER and NEVEN, effective temperatures,  $T_{\text{eff}}$ , were determined primarily by the slope of the energy distribution to the red of  $\lambda 4000$ ; effective gravities  $g_{\text{eff}}$  were found primarily from the Balmer discontinuity. It is believed that apart from uncertainties in the theoretical models,  $\theta_{\text{eff}} = 5040/T_{\text{eff}}$  can be determined to within 0.005 and  $\log g_{\text{eff}}$  within 0.1 to 0.2. The final effective range of  $T_{\text{eff}}$  is  $(5900 \div 7200)^\circ\text{K}$  which, according to OKE, compares favorably with other lines of evidence.

Formula (39) was then used to determine  $(R_{p,i}/R_{p,0})$ ; and assuming  $R_p = R_r$ , comparison with the displacements  $(R_{v,i} - R_{r,0})$  derived from Sanford's radial velocity curve yielded for  $R_0$  (identified here with  $R_{\text{max}}$ )

$$R_0 (= R_{\text{max}}) = (8.3 \pm 0.7) R_\odot.$$

The general agreement in phase and amplitude is quite good.

Finally with  $(GM/R_0^2) = 475$ , which corresponds to  $M = 1.2 M_\odot$ , formula (38), in conjunction with the radial velocity curve differentiated to yield  $\ddot{R}$ , was used to compute theoretical  $g_{\text{eff}}$ 's. The comparison between these and the observed values is also very satisfactory. The value of  $Q_0$  in the relation (19) comes out of the order of 0.03, using a mean radius  $R = 7.8 R_\odot$ .

For  $\eta$  *Aquilae*, the method followed was essentially the same, using a correction for interstellar reddening of 0.14 in the B-V scale as determined by KRAFT. A comparison between the observed absolute distribution of the flux in the true continuum (corrected for absorption lines), and that in model atmospheres computed by CANAVAGGIA and PECKER, yielded the values of  $T_{\text{eff}}$  and  $g_{\text{eff}}$ . The temperature range found in this way is from 5320 K to 6140 K.

The comparison between the radii derived from the radial velocity curve and those derived as shown above from relation (39) with a value of the minimum radius (taken here as the standard  $R_0$ )

$$R_{\text{min}} = 52.3 \pm 2.1 R_\odot$$

shows a good overall agreement in phase and amplitude. The value of  $M_v$  turns out to be equal to  $-3.85 \pm 0.3$  at mean light which is in good agreement with Kraft's value of  $\sim -3.6$ .

The essential difference with most of the previous treatments is the smaller range in  $T_{\text{eff}}$  and it is this, as already noted by GRANDJEAN and LEDOUX, which brings the agreement between the two radii.

The most obvious conclusion is that ordinary stellar model atmospheres computed at each phase, assuming radiative and hydrostatic equilibrium, seem to reproduce the observations and that there is no need to distinguish very carefully between the photospheric radius  $R_p$  and the radius  $R_r$  of the level where the weak absorption lines are formed.

Since the models used are computed for an assumed constant total flux through the atmosphere, it would seem difficult to reconcile these conclusions with the passing through the atmosphere of a strong isothermal shock-wave, which would act as a local moving heat source, creating a difference between the flux  $F_1$  on the external side and  $F_2$  on the internal side of the order of (cf. ref. A, section 92)

$$(40) \quad F_1 - F_2 = \frac{p_1}{2} \left( 1 + \frac{p_2}{p_1} \right) (v_2 - v_1),$$

which can become quite appreciable. Let us note too, in that respect, that in the case of *RR Lyrac*, OKE has observed no extra continuous emission in the U-V during the abrupt ascending branch phase but, in this respect, his observations may not refer to the most favorable moment in the 41-day cycle. The non-linear transfer of momentum associated with such a wave would also affect the effective gravity  $g_{\text{eff}}$ . However these investigations relate to stars with continuous velocity curves and no or very weak emission lines (except *RR Lyrac*, at some phases) and the conclusions may not necessarily apply to stars like *W Virginis*.

A few other hydrodynamical inferences can also be drawn from this type of investigation. Once model atmospheres have been fitted at a series of phases as described above, they can be used to follow, throughout a cycle, the Lagrangian variations in density of a given element defined by the constant mass above it.

In the case of  $\eta$  *Aquilae*, results derived by this method (LEDoux and GRANDJEAN, 1955) were in good qualitative agreement with those obtained previously by M. and B. SCHWARZSCHILD and W. S. ADAMS (1948) from a quantitative discussion of the intensities of the lines of neutral and ionized iron at 20 selected phases. In both cases, the variation of the density showed a much better agreement in shape and phase with the velocity curve than with the displacement curve, suggesting a predominant progressive character for the wave. However, as shown by these authors, a straightforward inter-

pretation on this basis leads to an impossibly high speed of propagation in the atmosphere.

This led them to consider a composite atmosphere with a hot corona, in which the wave can travel outward at high speed. Although, on this picture, the wave becomes again stationary at some depth in the atmosphere, there is an intermediate layer where it resembles that in the corona and, assuming that the observations refer to that level, the above explanation may then become acceptable.

However, apart from the fact that this is based on a linear theory, it is likely that a more realistic treatment would show that this layer is extremely shallow and situated at such a small optical depth that it cannot affect the lines. Furthermore, this explanation could certainly not be extended to the results derived from model atmospheres, since some of them refer to particles at fairly great optical depths. In this case, a comparison between different particles shows that the phase of the density variation could be explained by very small variations with height of the phase and (or) the amplitude of the velocity curves, which may correspond mainly to an increase of the anharmonicity with height.

**3'2. Evidence from the line spectrum.** — The bifurcation, through Doppler shifts, of the spectral lines of *W Virginis*, *RR Lyrae*, and  $\beta$  *Cephei* stars is convincing evidence for the existence of very steep gradients in their atmospheres. In fact the presence of emission lines associated with upward-moving gas indicates the existence of a high temperature region such as might be associated with a shock discontinuity.

Unfortunately the data available are grossly inadequate for the determination of the actual velocity fields. The principal limitation is the fact that an emergent pencil of radiation integrates information from a wide range of atmospheric levels and conceals the details of its source. Furthermore, the purity of spectra presently available is insufficient to allow a precise determination of the profiles of spectrum lines.

In sum, the *a priori* expectation that valuable data on the velocity field might be obtained from profiles and velocity-shifts of spectral lines has not been fulfilled.

In view of the paucity of reliable data, there seems little point in interrupting this text with a discussion of discordances between the opinions of various astronomers. We shall however abstract some relevant papers in the Appendix.

R. G. TESKE (1960) has constructed *theoretical* line profiles for pulsating atmospheres with and without velocity gradients. His aim was to provide a theoretical framework within which to examine available data and to suggest what is desired from future observations.



Two of his results are of particular significance for the interpretation of spectroscopic data on pulsating atmospheres.

He has shown that the curve of growth for lines formed by pure absorption is affected by a velocity gradient in a manner which roughly mimics the effect of microturbulence.

Line profiles produced by a pulsating atmosphere are asymmetric, since the line-wings are formed at greater depths than the line-centers. An asymmetry will exist whether or not there is a vertical gradient of velocity. In the absence of such a gradient, this asymmetry is produced by the fact that observed radiation represents an integration over the spherical stellar disc. We shall designate the asymmetry arising in this case as the «zero-gradient asymmetry.»

TESKE has re-examined earlier work on the observed line asymmetries and the velocity differences between lines of different strengths, and he concludes that these two problems are quite closely related.

There are several cases (*e.g.* the *RR Lyrae* variables) in which large velocity differences are definitely observed. These can amount to several tens of km/s. and are most pronounced between the hydrogen lines and the weak iron lines. The interpretation of these differences in terms of velocity gradients has not yet been put on a quantitative basis.

In the case of small velocity-differences, the problem is complicated by the subjective nature of the data reduction. The published line-shifts are based on micrometer measures of the «position» of absorption lines. For asymmetric lines, the results will be different for the center of gravity, the minimum of intensity, or the midpoint of the wings of the absorption lines. Unfortunately, it is not clear which of these positions is actually measured by an individual measurer.

On the assumption that the micrometer measures refer to the deepest point of the line profile, TESKE shows that apparent differential velocities will be recorded even in the zero-gradient case. Further, these differences will be a function of the depth of line formation and will therefore mimic the effects of a velocity gradient. TESKE concludes that many of the «velocity differences» discussed in the literature may be produced by zero-gradient asymmetries.

Evidently more precise and objective data are needed before we can specify the origin of these small velocity differences.

#### 4. – Some theoretical aspects of the dynamical behavior of stellar atmospheres.

In view of the virtual impossibility of a direct empirical approach to the determination of atmospheric velocity structure, we shall now consider the theoretical approach through synthesis.

To provide a foundation for this discussion, we first describe some properties of stellar atmospheres relevant to their dynamical behavior.

4.1. *Properties of the undisturbed stellar atmosphere.* — The differential equations governing the dynamics of stellar atmospheres contain a set of dimensionless parameters whose enumeration is useful.

Let

$H = \mathcal{R}T/\mu g$  = atmospheric scale-height,  $\mu$  = mean molecular weight,

$c = \sqrt{\gamma \mathcal{R}T/\mu}$  = sonic velocity in the atmosphere,

$g$  = stellar surface gravity,

$P$  = period of pulsation,

$p$  = gas pressure.

Then the dimensionless ratio of scale-height to photon free path is  $H\kappa_0$  where  $\kappa$  is the absorption coefficient and  $\rho$  the gas density. Although the scale-height does not vary rapidly through the atmosphere, the density does; so, consequently, does  $H\kappa_0$ . Defining the optical depth of a layer through

$$\tau = - \int_x^\infty \kappa \rho \, dx,$$

and utilizing the approximate constancy of  $H$ , we see that  $H\kappa_0$  at any level is roughly equal to the optical depth of the level. For the observable atmosphere, therefore,  $H\kappa_0 \simeq 1$ .

A thermal parameter may be constructed in the following way. The quantity  $\kappa_0 \sigma T^4$  is, for a «grey» gas, the rate of emission from an optically thin volume element. The ratio  $p/\kappa_0 \sigma T^4$  is the time to radiate the thermal energy of this element at constant temperature. The ratio  $H/c$  is the time for a small perturbation to travel one scale height. Therefore the order of magnitude of the following parameter,  $\alpha_t$ , indicates the extent to which an optically thin perturbation will be affected by radiation while travelling through the atmosphere

$$\alpha_t = \frac{\kappa_0 \sigma T^4}{p} \frac{H}{c}.$$

Using  $H\kappa_0 \simeq 1$ , this simplifies to

$$\alpha_t = \frac{\sigma T^4}{\rho c}.$$

This parameter may be regarded as an index of departure from adiabaticity. Adopting a hydrogen atmosphere at  $T=5\,000^\circ$ ,

$$\alpha_T \simeq \frac{5 \cdot 10^3}{p},$$

and since  $10^3 < p < 10^4$  for typical pulsating atmospheres at  $\tau=1$ , we see that  $\alpha_T$  is of the order of unity. Thus, deviations from a radiative equilibrium distribution of temperature tend to smooth out in a time comparable to the acoustic delay for one scale height.

TABLE I. — *Physical properties of three pulsating stars.*

	$\delta$ Cephei	RR Lyrae	W Virginis
$P$ (days)	5.4	0.57	17.3
$R$ (cm)	$3.7 \cdot 10^{12}$	$4.6 \cdot 10^{11}$	$4.4 \cdot 10^{12}$
$g$ (cm/s <sup>2</sup> )	100	800	10
$T$ (°K)	6000	6800	6000
$H$ (cm)	$8 \cdot 10^9$	$1.2 \cdot 10^9$	$1 \cdot 10^{11}$
$\Delta v$ (km/s)	40	100	55
$c$ (km/s)	9.1	9.6	9.1
$\alpha_\lambda$	50	40	14
$\alpha_D$	.086	.25	.37
$(\sigma/\pi)T^4$	$2.4 \cdot 10^{10}$	$3.9 \cdot 10^{10}$	$2.4 \cdot 10^{10}$

A geometrical parameter of interest is the ratio of pulsation period to  $H/c$ , the acoustic delay time of one scale height, *i.e.*

$$\alpha_\lambda = \frac{Pc}{H}.$$

This quantity may also be considered as the ratio of the atmospheric pulsation wavelength to the atmospheric scale-height.

This quantity is also of interest in conjunction with  $\alpha_T$  in indicating the extent to which pulsational disturbances of the temperature from the radiative distribution are suppressed by radiative transfer. Values of  $\alpha_\lambda \gg 1$ , such as are tabulated above, indicate that, taken grossly, the temperature distribution in a pulsating atmosphere is not very different from the equilibrium distribution. It must, of course, differ quite significantly during the brief intervals when steep velocity gradients lie near  $\tau=1$ .

As an index of the degree of departure from hydrostatic equilibrium we may construct the dynamical parameter  $\alpha_D$  through

$$\alpha_D = \frac{v}{Pg},$$

where  $r$  is the amplitude of the observed pulsation velocity-curve. Writing  $v = Mc$ , where  $M$  is the Mach number, and using

$$H = \frac{c^2}{\gamma g},$$

we find

$$\alpha_D = \frac{M\gamma}{\alpha_\lambda}.$$

In Table I we list some of the stellar dimensions relevant to the present discussion for three prototype stars. The stars are listed in order of increasing  $\alpha_D$  and increasing  $\alpha_\lambda$ . It is interesting to note that, observationally, this sequence is also one of increasing evidence for discontinuity in the atmospheric velocity distribution. That is,  $\delta$  *Cephei* shows no discontinuity, *RR Lyrae* shows it only in the strongest, high-level, lines and *W Virginis* shows it for all lines. This coincidence may be interpreted in the following way (WHITNEY, 1955).

A large value of  $\alpha_D$  indicates that the atmosphere is driven violently by the interior pulsations and deviates significantly from hydrostatic equilibrium. Thus a large  $\alpha_D$  should be propitious for the production of velocity discontinuity.

In a crude way, the smaller the value of  $\alpha_\lambda$ , the greater is the distance in wavelength that the pulsation wave must travel in traversing the atmosphere. This is also propitious for the development of shocks.

Thus, the sequence of increasing discontinuities is consistent with the related observational parameters.

**4.2. Outline of the dynamical problems.** — These problems may be listed as follows:

a) The development of a shock discontinuity from a continuous wave of moderate amplitude.

We face here the problem of relating the atmospheric wave to the conditions at the exterior limit of the nearly-adiabatic, small-amplitude pulsation of the interior. This conceptual dichotomy between the interior and the atmosphere is quite artificial, but the fact remains that it can be useful since the differential equations governing most of the interior are considerably simpler than those for the atmosphere. One specific example of the «useful misuse» of this dichotomy has been the replacing of the interior-atmosphere transition region by a solid oscillating piston. This model provides a handy boundary condition for the treatment of atmospheric waves (WHITNEY, 1956) but must give an inaccurate picture of the development of atmospheric shocks.



b) The derivation of conditions immediately behind the shock and the behavior of the ionizing hydrogen within the shock transition.

c) The description of the physical nature and spectroscopic properties of the regions of recombination and radiation behind the shock.

d) A determination of the overall temperature distribution and radiation field in an atmosphere containing a radiating shock.

Of particular interest in this regard is the effect of radiation leakage from the shock into the atmosphere lying ahead of the shock.

e) The growth or decay of a strong shock moving up along a density gradient.

In the next section we write the non-linear equations applicable to atmospheric waves and in succeeding sections we attempt preliminary discussion of some of the questions outlined above.

4'3. *The relevant equations.* — We shall restrict ourselves to the one-dimensional case, since the depth of the stellar region of interest here is considerably less than its radius of curvature. We also neglect viscosity.

In their Eulerian form, the conservation equations may be written as follows for the one-dimensional case.

Conservation of mass:

$$(41) \quad \frac{\partial \varrho}{\partial t} = - \frac{\partial}{\partial x} \varrho v .$$

Conservation of momentum:

$$(42) \quad \frac{\partial}{\partial t} \varrho v + \frac{\partial}{\partial x} \varrho v^2 = - \frac{\partial p}{\partial x} - g \varrho .$$

Conservation of energy:

$$(43) \quad \begin{aligned} \frac{\partial}{\partial t} \left( \frac{3}{2} p + E_i + \frac{1}{2} \varrho v^2 \right) = & \\ = - \frac{\partial}{\partial x} p v - g v \varrho - & \quad \text{(work)} \\ - \frac{\partial}{\partial x} v \left[ \left( \frac{3}{2} p + E_i + \frac{1}{2} \varrho v^2 \right) \right] + & \quad \text{(convection)} \\ + Q_r + Q_c , & \quad \text{(radiation and conduction).} \end{aligned}$$

The conservation of energy requires that the rate of change of energy per unit volume equal the sum of the rates at which work is done on the element and energy is carried into the element by mass motion, radiation and conduction.  $E_i$  is the density of excitation and ionization energy and  $g$  is the gravitational acceleration, which is directed toward negative  $x$ .

Radiative transfer:

$$(44) \quad \cos \theta \frac{\partial I_\nu(x, \theta)}{\partial x} = -\kappa_\nu \varrho I_\nu + j_\nu \varrho.$$

$I_\nu(x, \theta)$  is the specific intensity of radiation at  $x$  in a direction making an angle  $\theta$  with the  $x$  axis,  $\kappa_\nu$  and  $j_\nu$  are the mass absorption and emission coefficients.

Integration of the transfer equation over all solid angles and all frequencies leads to the following expression for  $Q_r$ , the radiative input term

$$(45) \quad Q_r = 2\pi \frac{d}{dx} \int_0^\infty d\nu \int_{-1}^1 I_\nu \cos \theta (d \cos \theta) = 4\pi \varrho \int_0^\infty (\kappa_\nu \bar{I}_\nu - j_\nu) d\nu,$$

where the bars denote mean values with respect to direction.

It may be convenient to cast  $Q_r$  into another form which is obtained by formal integration of the transfer equation. Define the differential of optical depth by

$$(46) \quad d\tau_\nu = -\kappa_\nu \varrho dx,$$

divide the transfer equation by  $\kappa_\nu \varrho$  and integrate. The following expression then results,

$$(47) \quad Q_r = \frac{d}{dx} \int_0^\infty F_{\nu r} d\nu,$$

where

$$(48) \quad F_{\nu r} \equiv 2\pi \int_\tau^\infty S_\nu(t) E_2(t - \tau) dt - 2\pi \int_0^\tau S_\nu(t) E_2(\tau - t) dt,$$

$$(49) \quad S_\nu \equiv j_\nu / \kappa_\nu.$$

4.1. *The isothermal equations and a kinematic model for W Virginis.* — Temperature fluctuations in a stellar atmosphere are smoothed very rapidly by radiative transfer of energy. This fact, and the great simplification thereby introduced, made it appropriate to adopt the isothermal form of the dynamical equations for preliminary theoretical studies of atmospheric pulsations.

The use of the isothermal approximation clearly eliminates the possibility of directly synthesizing the atmospheric thermal structure and radiation field. However the purely kinematical results have been useful in two ways. On the one hand, they have provided a model with which to interpret several aspects of the velocity curves of *W Virginis* stars. On the other hand they provide an approximate velocity field with which to study the relative importance of the compressional work term and the radiation term of the energy equation.

The differential equations obtained by setting  $T = \text{constant}$  may be directly integrated from adopted boundary conditions. The integration is, however, subject to numerical instability associated with the generation of parasitic and exponentially-growing oscillatory solutions of the finite-difference equations (Courant-Friedrichs instability). These oscillations may be suppressed over most of the flow by proper choice of mesh intervals, but they inevitably appear in regions of steep gradients.

A more powerful method of eliminating this difficulty is through the use of the method of characteristics.

From eq. (41) and (42), *i.e.*,

$$\begin{aligned}\frac{\partial \varrho}{\partial t} + \frac{\partial}{\partial x} \varrho v &= 0, \\ \frac{\partial v}{\partial t} + v \frac{\partial v}{\partial x} &= -\frac{1}{\varrho} \frac{\partial p}{\partial x} - g,\end{aligned}$$

we may derive the dimensionless equations

$$(50) \quad \frac{\partial}{\partial \tau} R + (u + 1) \frac{\partial}{\partial \eta} R = 0, \quad R = u + \ln \varrho + \tau;$$

$$(51) \quad \frac{\partial}{\partial \tau} S + (u - 1) \frac{\partial}{\partial \eta} S = 0, \quad S = u - \ln \varrho + \tau.$$

Where we have defined the new variables through

$$\begin{aligned}\tau &= t \frac{c}{H}, \\ \eta &= x/H, \\ u &= v/c.\end{aligned}$$

The density is in units of the density at some convenient point, *e.g.*, ( $x=0$ ,  $t=0$ ).

Evidently  $R$  and  $S$  are constant along lines of slope  $u+1$  and  $u-1$ , respectively. Further, the values of  $u$  and  $\varrho$  may be derived from  $R$ ,  $S$  and  $\tau$

through inversion of the defining relations. The shock transitions are handled with the Rankine-Hugoniot relations, and the integrations proceed in a straightforward manner.

Results of integrations for three sets of boundary conditions have been published (WHITNEY, 1955). These integrations started from  $r = 0$  throughout the gas for  $t < 0$ . A piston at the bottom of the atmosphere was stationary for  $t < 0$  and driven sinusoidally with a velocity amplitude equal to sonic velocity for  $t > 0$ . The atmosphere was assumed to extend infinitely far upwards.

The initial shock developed quite close to the piston and travelled out into the stationary atmosphere with an increasing velocity. The motion of subsequent shocks was complicated by the preceding disturbances of the atmosphere and the integration were, perforce, stopped after several piston oscillations. By this time, the flow within several scale heights of the piston had apparently relaxed to a nearly cyclic state, although at greater heights the transients associated with the onset of pulsation still dominated the flow.

It is, of course, very dangerous to draw any conclusions about the cyclic flow in pulsating atmospheres from these limited «initial value» solutions. However, two features of the results are worth mentioning.

First, these large amplitude waves transferred momentum to the atmosphere, «levitating» it and reducing the average density gradient by a factor four. We may expect a similar effect in a real pulsating atmosphere. However, the piston frequencies employed in these integrations were higher than are appropriate for real atmospheres. Lautman's results (1956) indicate that at lower frequencies the effect may be less pronounced.

Second, the atmospheric density distribution, which would have resembled a *N*-wave if the initial medium had been homogeneous, has a stairway profile. That is, the density was nearly constant, at any given time, between succeeding shock discontinuities.

The following kinematic model was synthesized from these partial results and applied to *W Vir* (WHITNEY, 1956):

- 1) Shock fronts are generated and they travel upward with constant strength.

- 2) All particles between shocks are subjected to the same downward acceleration, and this acceleration is independent of time. Thus, particle trajectories are parabolic on the space-time plane. In passing through a shock front, each particle has its velocity impulsively reversed, but its speed unchanged.

On this model, the shock velocity is constant with respect to the center of mass of the star as well as with respect to the matter flowing into it from above.



Four quantities specify the model:

a) The velocity of shock propagation. This is chosen so that the velocity discontinuity at the shock is compatible with the amplitude of the observed velocity curve.

b) The oscillation period in dimensionless units. This parameter is equivalent to  $\alpha_\lambda$ , defined in Section 41.

c) The downward acceleration. This quantity is derived from the previous two and the requirement that there be no net flow of matter.

These quantities specify the velocity field. From the continuity equation and the fourth quantity, the density at an arbitrary point, the density distribution may be determined for the entire atmosphere.

Relation of this model to the atmosphere of *W Virginis* entailed a lengthy process of successive approximations to the spatial distribution of temperature, total pressure, electron pressure and optical depth. Empirical data were taken from ABT's (1954) study.

The most serious approximation involved in construction of this model is that the relation between temperature and optical depth is that appropriate to radiative equilibrium. The two sources of departure from radiative equilibrium are 1) the time variations of radiant flux from the stellar interior into the bottom of the atmosphere and 2) the conversion of kinetic energy into thermal energy represented by the work terms in the energy equation.

The second of these effects will be important over a limited interval of depth within the atmosphere. That is, the radiation produced by compressional heating across a shock discontinuity will lead to significant departure from radiative equilibrium in the immediate neighborhood of the shock. We have not yet investigated this situation and cannot apply the present model to *W Virginis* during those phases when the shock is traversing the observable atmosphere.

However, when the shock has passed above the region of spectrum formation or before it has entered this region from below, the assumption of radiative equilibrium will not be grossly in error. The compressional term in the energy equation will then be negligible in the region of spectrum formation.

Also calculation of the relaxation of the temperature distribution in a static atmosphere (WHITNEY, 1955a) after an abrupt change of the radiant flux from below indicates relaxation times of the order of an hour or less. This is a small fraction of the pulsation period for *W Virginis* stars (10 to 20 days), so that the variation of the radiant flux into the bottom of the atmosphere will not in itself lead to significant departures from radiative equilibrium.

One specific result of the construction of this model for *W Virginis* of particular value relates to the velocity dispersion within the region of line formation. In sum the situation is the following:

When the shock rises into the visible portion of the atmosphere, the pressure and opacity in the rising material are very high. Therefore the dispersion of pulsation velocity within the visible material is small. At this phase we observe spectrum lines from matter above and below the shock and doubled lines are produced. As matter streams down through the rising shock the total optical thickness of the overlying atmosphere diminishes, finally becoming so small that this matter becomes invisible. Concurrently, the opacity of the rising gas behind the shock decreases and a wide region behind the shock contributes to the spectrum. Within this region of spectrum formation the dispersion of pulsation velocity becomes as large as 10 to 20 km/s.

This dispersion of pulsation velocity will broaden and strengthen the absorption lines in much the same manner as «micro-turbulence». Such an effect has indeed been detected by ABT (1954) although he has interpreted it as a real turbulent wake behind the shocks. The present model suggests that the observations can be interpreted quite naturally in terms of the gradient of the pulsational velocity.

All that can conservatively be said about the proposed model for *W Virginis* is that it is not in obvious conflict with available data, it reproduces the gross feature of the observed velocity curve, and it has given some insight into the structure of a pulsating atmosphere.

**4.5. Previous work on the structure of shocks in the presence of radiation.** — In this section we shall comment briefly on published investigations concerning the effects of radiation on flow behind shocks.

This work has been carried out with the assumption of asymptotic approach to a uniform medium at great distances from the shock. The conservation equations may be written in the steady form, dropping the explicit time-dependence,

$$(52) \quad \frac{d}{dx} \varrho v = 0,$$

$$(53) \quad \frac{d}{dx} (\varrho v^2 + p) = 0,$$

$$(54) \quad \frac{d}{dx} \left\{ r \left( \frac{3}{2} p + E_i + \frac{1}{2} \varrho v^2 \right) \right\} - \frac{d}{dx} p r - Q = 0,$$

where  $E_i$  designates the energy associated with internal degrees of freedom.

Setting  $E_i = 0$  and integrating over a discontinuity of infinitesimal thickness leads to the usual Rankine-Hugoniot (R-H) relations.

SACHS (1946) has introduced radiation pressure and energy density, writing

$$p = p_g + \frac{1}{3} a T^4$$

$$E_i = a T^4$$

and explicitly neglecting transfer of energy by radiation. He has derived the resulting transition relations, but since radiative transfer is important in the stellar atmosphere his results are not applicable to the present problem.

SEN and GUESS (1957) and MARSHAK (1958) have attempted to include radiative transfer and they evaluated  $Q$  from the radiative diffusion approximation. They neglected the radiative contributions to pressure and energy density. The former authors were interested in the structure of the shock transition, but the diffusion approximation

$$F_r = - \frac{4acT^3}{3\kappa_0} \frac{dT}{dx}$$

is not appropriate for such a study. The condition for applicability of the diffusion approximation, *i.e.* that the temperature does not vary significantly over distances comparable to  $1/\kappa_0$ , the photon mean-free-path, cannot be fulfilled in the region of the shock transition.

The radiative term must be evaluated properly from the transfer equation. The obvious mathematical difficulty introduced by the radiative term is that conditions at any point are, in principle, influenced by conditions throughout the medium. The governing equations cannot rigorously be reduced to purely differential form but must contain an integral term.

In a recent investigation explicitly recognizing this feature of radiative transfer, KUBIKOWSKI (1959) has evaluated the energy term from eq. (48). However, his reduction of the resulting expression to a tractable form is incorrect and, as noted in the next section, his results are grossly in error.

**4.6. Some features of strong shocks in hydrogen.** — We wish now to investigate some properties of strong shock transitions in an atmosphere composed of hydrogen. Points of principal interest are *a*) the effects of ionization within the shock transition; *b*) the temperature distribution behind the shock in the presence of radiative cooling; and *c*) the effects of radiative transfer to the region ahead of the shock.

Our interest lies in shocks moving parallel to a negative density-gradient, so in principle we should not employ the steady-state equations derived for a medium which is homogeneous at great distances from the shock. However, since the thickness of the region of interest is generally much less than one scale height of atmospheric density, we shall take advantage of the great simplification afforded by assuming the medium to be homogeneous in front of the shock, neglecting the gravitational forces.

Although the phenomena across and behind the shock should be treated with a unified theory, it is convenient to consider separately the following regions: 1) a homogeneous region in front of the shock; 2) the transition region for the external degrees of freedom in which conduction and viscosity

determine the physical conditions; 3) the relaxation region for internal degrees of freedom in which ionization and excitation take place; 4) the region in which radiative cooling takes place.

Although these distinctions are artificial, they are convenient; and the only one whose validity may be seriously questioned is that between regions 3) and 4).

Region 2), in which the external degrees of freedom relax but the internal degrees remain unexcited, will be a region of high temperature, but it is unobservable astronomically. As the gas moves into region 3), ionization takes place, the temperature will drop, and the density will rise. Since we assume the gas in front of the shock to be neutral, no radiation can occur until excitation and ionization have commenced. We shall therefore assume that *no* radiation occurs until ionization equilibrium is established. This model represents a considerable simplification of the physical situation, but it should be adequate for the present survey. In particular we note that radiation from the shock will dissociate the gas entering the shock and this may have a profound influence.

Let subscript 1 designate the undisturbed gas flowing into the shock and subscript 3 designate the gas at the back of region 3) after ionization has taken place.

In a co-ordinate system moving with the shock front, the R-H transition relations for the steady-state may be written

$$(55) \quad \varrho_3 v_3 = \varrho_1 v_1,$$

$$(56) \quad p_3 + \varrho_3 v_3^2 = p_1 + \varrho_1 v_1^2,$$

$$(57) \quad v_3 \left( \frac{5}{2} p_3 + E_{i3} + \frac{1}{2} \varrho_3 v_3^2 \right) = v_1 \left( \frac{5}{2} p_1 + E_{i1} + \frac{1}{2} \varrho_1 v_1^2 \right).$$

$E_i$  is the density of ionization energy,  $E_i = N_e \chi$ , and we neglect excitation energy.

We shall reduce these equations with the following approximations.

1) The pre-shock gas is not ionized, so  $E_{i1} = 0$ .

2) The shock is sufficiently strong that we may set  $\varrho_1 v_1^2 \gg p_1$ . This is equivalent to assuming that the square of the Mach number,  $M$ , is much greater than unity. For cases of astronomical interest  $10 < M^2 < 100$ , so we indeed deal with strong shocks.

3) For the strong shock we also have  $\varrho_1^{\frac{1}{2}} v_1^2 \gg \varrho_3 v_3^2$ . From these approximations we may derive

$$(58) \quad p_3 = \varrho_1 v_1^2.$$

$$(59) \quad \begin{aligned} \frac{5}{2} p_3 v_3 + N_e \chi v_3 &= \frac{5}{2} \varrho_1 v_1^2 v_3 + N_e \chi v_3 \\ &= \frac{1}{2} \varrho_1 v_1^3. \end{aligned}$$



If  $p_1$ ,  $v_1$ , and  $\varrho_1$  are chosen we may solve for conditions behind the shock provided we employ the ionization relation  $N_e = N_e(p, T)$ . For we may write

$$(60) \quad v_3 = \frac{\frac{1}{2} \varrho_1 v_1^3}{\frac{5}{2} \varrho_1 v_1^2 + N_e \chi},$$

and with the equation of continuity we find

$$(61) \quad \varrho_3 = \varrho_1 \frac{v_1}{v_3} = \frac{\frac{5}{2} \varrho_1 v_1^2 + N_e \chi}{\frac{1}{2} v_1^2}.$$

We find  $p_3$  from eq. (58) and guess a value of  $T_3$ . From eq. (61) and the ionization relation we find  $\varrho_3$ . The value so derived must satisfy the equation of state for pure hydrogen

$$p = N_e kT + \mathcal{R} \varrho T$$

and we adjust  $T_3$  until a self-consistent set of  $\varrho_3$ ,  $v_3$ ,  $T_3$ ,  $p_3$  is obtained.

Results of a set of calculations for conditions representative of classical cepheids are given in the table below.

TABLE II. — Shock transition in pure hydrogen

$T_1 = 5000^\circ, \quad \log p_1 = 3.0, \quad \log \varrho_1 = -8.62,$							
$v_1$	$M_1^2$	Temperature		$\log (p_3/p_1)$	$\log (\varrho_3/\varrho_1)$	$\alpha$	$\log N_e$
km/s		$T_2 \cdot 10^{-3}$	$T_3 \cdot 10^{-3}$				
30	13	25	10	1.33	.96	.15	15.32
40	23	41	11.5	1.58	1.08	.36	15.81
50	36	61	13.2	1.78	1.15	.61	16.11
60	52	86	15.5	1.94	1.17	.90	16.29
65	61	100	27	2.01	1.11	.99	16.27
70	71	116	27.5	2.07	2.07	1.0	16.19

Assumed conditions in front of the shock are given at the head of the table. The quantities  $M_1^2$  and  $\alpha$  are defined by

$$M_1^2 = \frac{v_1^2}{c_1^2}, \quad c_1^2 = \frac{5}{3} \mathcal{R} T_1,$$

$$\alpha = \frac{N_e}{N_H + N_e}, \quad N_H = \text{density of neutral hydrogen}.$$

Two values are given for the temperature corresponding to each velocity.  $T_2$  is the kinetic temperature immediately behind the shock before the onset of ionization. This temperature is computed from the R-H relations for a non-ionizing gas with  $\gamma = \frac{5}{3}$ .  $T_3$  is the temperature to which the gas relaxes before radiation occurs but after the ionization takes its equilibrium value.

These tables may be summarized as follows.

1) The density ratio between region 3) and 1) is about ten and is insensitive to the velocity. For velocities higher than those tabulated this ratio will fall to the value four appropriate to a non-ionizing gas.

2) Due to the energy sink provided by ionization, the temperature rise is decreased by a factor of from 2.5 to 5 within the range of this table.

3) Despite the ionization, the pressure ratio increases roughly as  $M_1^2$  and is about 30 per cent greater than the value corresponding to no ionization.

We turn now to the structure of region 4) in which radiative loss of energy proceeds.

We can make a preliminary estimate of the dimensions of region 4) in the following manner. Assume that the loss of energy takes place at constant pressure and temperature and neglect all terms in the energy equation (13) except

$$(62) \quad \frac{d(vE_i)}{dx} = Q.$$

The isothermal assumption is suggested by the large heat reservoir provided by the ionization energy.

The velocity entering this expression is relative to the shock; we designate it by  $v_4$  and assume it to be constant. Then eq. (62) becomes

$$(63) \quad c_4 \chi \frac{dN_e}{dx} = Q.$$

Finally, neglect energy *absorbed* in the cooling region, and for the emission term we neglect all but the free-bound emission, setting

$$Q = - .535 \cdot 10^{-21} \frac{N_e^2}{T^{\frac{1}{2}}} \text{ erg s}^{-1} \text{ cm}^{-3}.$$

Letting  $N_{e_1}$  be the electron density at the beginning of the radiating region,

and  $N_e$  the electron density at a distance  $\Delta x$  from this point, we find

$$\frac{\Delta x}{v_4} = 4.0 \cdot 10^{10} \frac{T^{\frac{1}{2}}}{N_{e_i}} \left( \frac{N_{e_i}}{N_e} - 1 \right).$$

We assume  $v_4 \simeq v_3 = v_1 \varrho_1 / \varrho_3$ .

Arbitrarily setting  $N_{e_i} / N_e = 11$ , we may define the characteristic time,  $\tau$ , and the characteristic thickness  $L$  as those dimensions corresponding to virtually complete radiation of the ionization energy. Then

(64) 
$$\tau = \frac{\Delta x}{v_4} = 4 \cdot 10^{11} \frac{T^{\frac{1}{2}}}{N_{e_i}},$$

(65) 
$$L = \tau v_4 = \tau v_1 \varrho_1 / \varrho_3.$$

From the results given in Table II we derive the characteristic dimensions given in Table III.

TABLE III.

$v$	$\tau$ (s)	$L$ (m)
30	.02	60
50	.003	9
70	.004	25

For the purpose of comparison with recent work by KUBIKOWSKI (1959), we have applied these equations to the conditions examined by him. The resulting cooling time is two orders of magnitude shorter than Kubikowski's value of 180 seconds.

We now proceed with a more detailed analysis of region 4) through numerical integration of the conservation equations.

We construct a one-dimensional space mesh labelling the points with index  $i$ , such that

(66) 
$$x_i = i \Delta x, \qquad (i = 3, 4, \dots).$$

The initial point  $x_3$  is identified with the rear of region 3), *i.e.*, the point at which ionization equilibrium has been established and radiation commences. The mass and momentum equation may be integrated to give, again for the steady-state,

(67) 
$$\varrho_{i+1} v_{i+1} = \varrho_i v_i,$$

(68) 
$$p_{i+1} + \varrho_{i+1} (v_{i+1})^2 = p_i + \varrho_i (v_i)^2.$$

Define the quantity  $u$  through

$$(69) \quad u = \frac{5}{2}p + E_i + \frac{1}{2}\rho v^2.$$

The energy equation then integrates to

$$(70) \quad v_{i+1}u_{i+1} = v_i u_i - \int_{x_i}^{x_{i+1}} Q(x) dx.$$

The net quantity  $Q(x)$  is the net rate at which energy is absorbed, per unit volume, at the point  $x$ . For the present case,  $Q(x)$  may be considered as the difference between the following two rates:

1) The rate,  $I_{fb}$ , at which energy is lost from the element through radiative recombinations. We again adopt

$$(71) \quad I_{fb} = .535 \cdot 10^{-21} \frac{N_e^2}{T^{\frac{1}{2}}}.$$

2) The rate,  $A_{bf}$ , at which energy is absorbed from the radiation field by the inverse process of radiative ionizations. The evaluation of the ionization rate requires determination of the radiation field. This, in turn, involves integration over the radiation sources distributed throughout the atmosphere. However, since the shock region of interest is optically thin, we shall neglect the exchange of energy between regions of the shock, in a first approximation. We assume that the remaining atmosphere is isothermal and constitutes a radiation bath equivalent to a black-body at temperature  $T_1$ .

With this model we may evaluate the rate of ionization from the rate of recombination through the principle of detailed balancing. Write

$$(72) \quad A_{bf} = C(T_1)N_H,$$

and set

$$(73) \quad \begin{cases} I_{fb}(T_1) = A_{bf}(T_1), \\ N_e = N_p. \end{cases}$$

Then

$$C(T_1) = \frac{.535 \cdot 10^{-21}}{T_1^{\frac{1}{2}}} (N_e^2/N_H)_*.$$

The quantity  $(N_e^2/N_H)_*$  is a function of  $T$  alone and the asterisk denotes that it is to be evaluated from the Saha equation for the temperature  $T_1$ .



Combining these relations we have

$$(75) \quad Q = A_{bf} - I_{fo} = .535 \cdot 10^{-21} \frac{N_e^2}{T^{\frac{3}{2}}} \left\{ \frac{T^{\frac{3}{2}}}{T_1^{\frac{3}{2}}} \left( \frac{N_o^2}{N_H} \right)_* \frac{N_H}{N_o^2} - 1 \right\}.$$

Throughout most of the relaxation process, the gas departs significantly from equilibrium and

$$(76) \quad \frac{N_o^2}{N_H} \ll \left( \frac{N_o^2}{N_H} \right)_*.$$

PETSCHEK and BYRON (1957) have emphasized that significant differences between the electron and atom kinetic temperature can arise during relaxation to ionization equilibrium through electron-atom collisional ionizations behind a shock. This ionization process cools the electron gas so severely that the rate of approach to equilibrium behind strong shocks in argon is determined not by the collisional ionization cross-section but rather by the rate at which energy can be fed to the electron gas by elastic electron-atom collisions.

This cooling of the electron gas will be important in region 3) in which the hydrogen is being collisionally ionized. We do not explicitly treat region 3) in this discussion, however.

In the radiative cooling region under discussion here, the electron concentration is *decreasing* through recombination, and there is no strong tendency toward a difference between atom and electron temperatures <sup>(1)</sup>.

Integrations of the complete set of conservation equations have been carried over the radiative cooling regions behind the shocks of velocity 30 km/s, 50 km/s, and 70 km/s listed in Table IV. The corresponding initial conditions are listed below.

TABLE IV. - *Initial conditions for radiative cooling.*

$v_1$	$\rho$ (g/cm <sup>3</sup> )	$p$ (dyn/cm <sup>2</sup> )	$T$ (°K)	$N_o$	$x$	$v$ (km/s)
30	$2.19 \cdot 10^{-8}$	$2.14 \cdot 10^4$	10 000	$2.09 \cdot 10^{15}$	.15	3.29
50	$3.39 \cdot 10^{-8}$	$6.02 \cdot 10^4$	13 200	$1.29 \cdot 10^{16}$	.61	3.54
70	$2.57 \cdot 10^{-8}$	$1.18 \cdot 10^5$	27 500	$1.55 \cdot 10^{16}$	1.00	6.54

Profiles of the temperature, density and  $Q$  are given in Fig. 5-6 for these cases <sup>(2)</sup>. The pressure profile is not plotted since in all cases the pressure increased by less than ten percent during the cooling.

<sup>(1)</sup> The energy-dependence of the recombination cross-section will produce a very small difference between these temperatures, but we neglect it.

<sup>(2)</sup> Examination of these profiles shows that thermal conduction is completely trivial in this cooling region.

We recall the fact, noted above, that the collisional relaxation to ionization equilibrium also takes place at essentially constant pressure (cf. PETSCHKE and BYRON, 1957). Put in different terms, the pressure distribution, in the

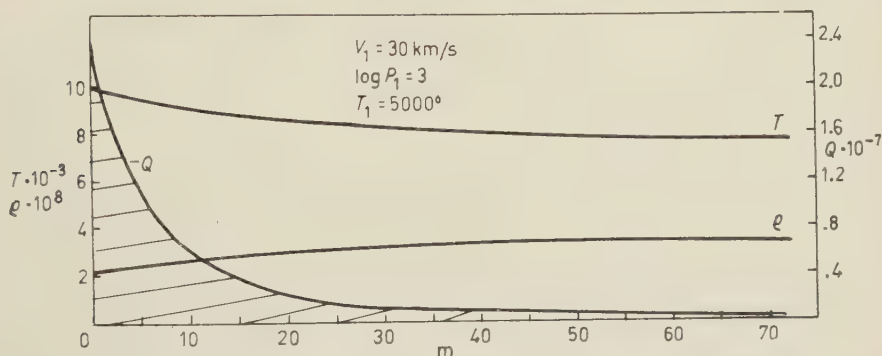


Fig. 5. - Spatial profiles of density, temperature, and emission rate behind a steady shock front in pure hydrogen. Shock velocity, 30 km/s; temperature and pressure in front of the shock, 5000 °K and  $10^3$  dyn/cm<sup>2</sup>, respectively. The coordinate system moves with the shock front, which is at the left border. Matter streams to the right.

presence of ionization and radiative recombination, is very nearly what it would be for a perfect gas in adiabatic flow.

These profiles indicate that for  $30 \text{ km/s} < v_1 < 50 \text{ km/s}$  and for the assumed initial pressure, eq. (65) gives a good approximation to the width of the cooling region. For  $v = 70 \text{ km/s}$  the cooling proceeds more rapidly than given by this equation. This occurs because the mean temperature is lower and the mean electron density is higher during cooling than assumed in eq. (65).

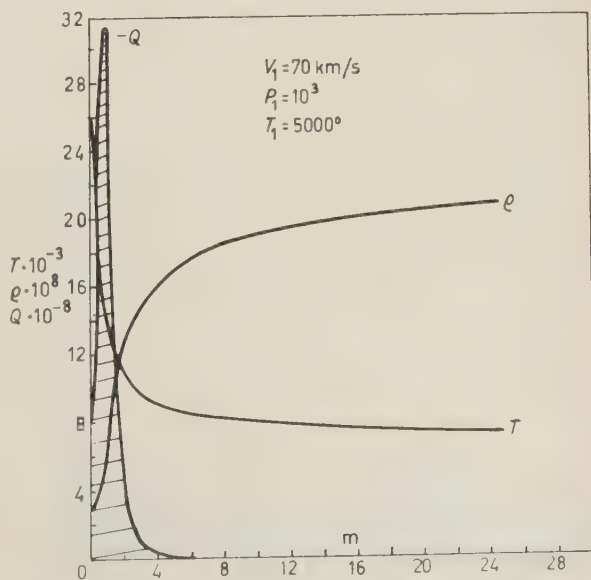


Fig. 6. - Same as Fig. 5 except that the shock velocity has been increased to 70 km/s. Note that the free-bound emission rate initially increases and then decreases with distance from the shock front. Also the thickness of the high-temperature region is reduced at the higher velocity.

Table V summarizes some pertinent properties of the cooling process.

Columns two and three give the enthalpy and the density of ionization energy immediately behind the shock. For the strongest shock these quantities become nearly equal.

TABLE V. — *Some properties of the radiating shocks.*

$v_1$	$\frac{5}{2}p$	$N_e$	$\frac{1}{2}\rho_1 v_1^3$	$\int Q dx$	$\dot{N}_\nu$	$\dot{N}_H$
30	$5.35 \cdot 10^4$	$4.6 \cdot 10^5$	$3.2 \cdot 10^{10}$	$1.9 \cdot 10^{10}$	$.5 \cdot 10^{21}$	$4.5 \cdot 10^{21}$
50	$1.50 \cdot 10^5$	$2.8 \cdot 10^5$	$1.5 \cdot 10^{11}$	$1.1 \cdot 10^{11}$	$3 \cdot 10^{21}$	$7.5 \cdot 10^{21}$
70	$2.95 \cdot 10^5$	$3.4 \cdot 10^5$	$4.2 \cdot 10^{11}$	$3.7 \cdot 10^{11}$	$1 \cdot 10^{22}$	$1.0 \cdot 10^{22}$

We have evaluated the total emission per  $\text{cm}^2 \cdot \text{s}$  from the shock and these values are labelled  $\int Q dx$ . As is expected, the total emission approaches  $\frac{1}{2}\rho_1 v_1^3$ , the rate of kinetic energy flow across the shock.

Finally the last two columns compare the flux of ionizing photons,  $\dot{N}_\nu$ , across the shock from behind, with the flux of hydrogen atoms,  $\dot{N}_H$ , across the shock from in front. We have assumed that the average photon energy,  $h\nu \simeq 14 \text{ eV}$ , and have written

$$\dot{N}_\nu = \frac{\int Q dx}{2h_\nu},$$

where the factor  $\frac{1}{2}$  accounts for the fact that this fraction of photons will be emitted toward the shock front.

If we make the limiting assumption that 1) the material in front of the shock is sufficiently opaque that all photons will produce ionization and 2) no recombination takes place ahead of the shock, we see that material streaming into the weaker shocks will receive only a slight additional ionization from the shock radiation. For the strongest shock, however, the material flowing into the shock will be highly pre-ionized.

The pre-ionization will have a profound influence on the structure of the shock. Therefore the present calculations for  $v_1 \geq 50 \text{ km/s}$  must be grossly incorrect and should be used simply to indicate the importance of pre-ionization.

## APPENDIX

We assemble here the best data available concerning the velocity fields in pulsating atmospheres. We shall restrict attention to data obtained with high-dispersion spectra, since results based on moderate dispersion are unreliable.

The literature being rather limited, we have included brief summaries of the relevant papers rather than summarizing all of the data in a single discussion. Comments by the present writers accompany several of the summaries.

## 1. - Summaries of papers on classical cepheids.

### 1. SANFORD, R. F.

Ref.: *Ap. J.*, **123**, 201 (1956).

Subject: Examination of 10 Å/mm 100" coude plates of two classical cepheids *SV Vul* ( $P = 45d$ ) and *T Mon* ( $P = 27d$ ).

Presents velocity curves for Fe I lines and residual for Fe II, Ti II, Sr II, H. The author makes two comments on Fe I curve.

1) Velocity extrema lag light extrema by

$$\begin{array}{ll} T \text{ Mon} & \Delta\Phi = 0.044 \text{ periods,} \\ SV \text{ Vul} & \Delta\Phi = 0.072 \text{ periods.} \end{array}$$

This lag and its increase with period had been previously noted, esp. by Joy (1937).

2) The velocity curves are very nearly repetitive.

Concerning differential velocities the author makes the following comments.

a) No significant  $\Delta v$  between Fe II and Fe I.

b) Hydrogen shows positive residual of 50 km/s just before light maximum, i.e. there is a phase lag of 5 days in maximum positive velocity relative to Fe I.

c)  $H_x$  differs from other  $H$  lines by plus 20 km/s during decline of light.

d) Velocity residuals of  $H$  and metallic lines are correlated with line widths and the author suggests that both are produced by an extension and deepening of redward line wings.

Although the line profiles are generally symmetric, the  $H_x$  and  $H_\beta$  profiles show redward displaced cores near maximum light.

Emission lines of Ca II are strong in *T Mon* from phases 22<sup>h</sup> 28 to 26<sup>h</sup> 895 but are not apparent at other phases. Interpretation of the emission is complicated by the presence of an interstellar absorption line.

### 2. KRAFT, R. P.

Ref.: *P.A.S.P.*, **68**, 137 (1956).

Subject: Examination of plates of *X Cyg*, a classical cepheid of period 16 days.

The author concludes that the absorption lines of low excitation potential are doubled at  $\Phi = 0^m.82$ , i.e., on the rising branch of the light curve. The separation of the Fe I line ( $\lambda 3923$ ) is 38 km/s. The  $H_\gamma$  core is displaced



42 km/s from the  $\lambda$  3923 line. The undisplaced component is not observed and the author suggests, by analogy with *W Vir*, that it has been obliterated by emission.

The author comments that the luminosity expected from the shock producing the line-doubling is comparable with the total luminosity of the star. The atmospheric shock may therefore produce the hump on the light curve.

### 3. GRANDJEAN, J.

Ref.: *Mem. Acad. Roy. Belgique, Cl. Sci.*, Coll. No. 8, vol. 22, fasc. 7 (1956).

Subject: A study of differential motions in the atmospheres of the classical cepheids  $\eta$  *Aql* and  $\zeta$  *Gem* based on 2.9 Å/mm Mt. Wilson 100" coude plates. Summarizes earlier work with the following comments:

« Tirer de toutes ces investigations une conclusion nette, n'est pas chose aisée....

L'aspect assez chaotique de ces observations est très explicable quand on pense que ces déterminations ont été faites à l'aide de spectrogrammes ayant une dispersion de 40 Å par mm à  $H_{\gamma}$ . A cette dispersion, la majorité des raies du spectre sont des « blends » non résolus et la variation d'intensité des composantes peut donner des déplacements apparents de la raie qui interprétés en termes d'effets Doppler, donnent des vitesses relatives n'ayant aucune réalité physique. Les travaux de JACOBSEN réalisés à plus grande dispersion, 13 Å par mm à  $H_{\gamma}$ , ne sont guère plus convaincants. Néanmoins, ils excluent les grands écarts prétendument mis en évidence précédemment, car, à cette dispersion (3 fois plus élevée), de tels effets devraient apparaître nettement, or, il n'en est rien. Quant à des effets moins importants, rien de certain n'est établi; la dispersion est toujours faible, les erreurs probables relativement grandes, et ce que nous avons dit précédemment au sujet des « blends » reste vrai en partie. »

The author measured radial velocities of a large number of lines using a Zeiss comparator and concludes that small velocity gradients definitely exist (amounting to  $(3 \div 4)$  km/s over the depth of formation of the lines measured). He comments that it is tempting to interpret these velocity differences as resulting from a slight phase lag of the higher layers relative to the lower but adds that an increase of velocity amplitude with height could also explain the differences.

On the basis of model atmospheres and calculated depths of line formation the author estimates that the velocity gradient is of the order of

$$1.5 \cdot 10^{-8} \text{ km/s km.}$$

*Comment.* — Recent theoretical work by R. G. TESKE indicates that line shifts dependent on line intensity can be produced in the spectra of stars even in the absence of real differential velocities.

### 4. JACOBSEN, T. S.

Ref.: *Pub. Dom. Ast. Obs.* 10, No. 6 (1956).

Subject: A spectrographic study of  $\eta$  *Aql* and  $\delta$  *Cep* based on plates of 20 Å/mm dispersion.

The author finds essentially no velocity differences among photospheric absorption lines. Further, the core of  $H_{\delta}$  seems to follow these lines closely. The Ca II  $H$  and  $K$  lines show large departure from the photospheric lines and also periodically show marked asymmetry.

In  $\delta$  Cep the Ca II  $K$ -line has a larger amplitude but nearly the same phase as the photospheric lines. In  $\eta$  Aql, the amplitude of the  $K$ -line velocity curve is greater than that of the photospheric curve and the  $K$ -line curve is distorted and appears to be retarded by about  $0^{\circ}.1$ .

## 5. HERBIG, G. H.

Ref.: *Ap. J.*, **116**, 369 (1952)

Subject: Study of 10 Å/mm 100" coude plates of  $S$  Sge ( $P = 8.4$  days) with special emphasis on emission lines of Ca II.

Comments that Ca II emission is present in many classical Cepheids. In  $S$  Sge it occurs during the middle of ascending branch of light curve and is transitory, lasting two days or less.

The author remarks that there is considerable evidence that the emission lines in long period (red) variables originate below the layers of formation of the cores of the absorption lines. He notes the following points of similarity between the long period variable emission and that of  $S$  Sge.

1) The emission is diffuse and displaced to the violet relative to the absorption lines.

2) An increase of absolute strength of the emission with phase is shown for  $S$  Sge and is apparent in the long period variables from measured relative intensities.

3) In the long period variables  $I(H) > I(K)$  of Ca II as opposed to the relative transition probabilities. This is an exaggeration of the tendency to equality  $I(H) \simeq I(K)$  in  $S$  Sge.

4) Ca II  $K$  emission shows a greater violet displacement than Ca II  $H$  in both types of stars.

The most significant differences between the phenomena in the two types of stars is the greater strength in the long period variables and the fact that emission occurs during the descending branch of the light curve.

The author discusses self absorption in a simple two-layer atmosphere and indicates that a low-lying origin of the emission can account for the presence of the Ca II infrared emission in the absence of detectable  $H$  and  $K$  emission.

## 6. SCHWARZSCHILD, M. and B., ADAMS, W. S.

Ref.: *Ap. J.*, **103**, 207 (1948).

Subject: A study of the spectrum of  $\eta$  Aql ( $P = 7.2$  days) based on 2.9 Å/mm plates.

Measurements of line cores show very little evidence for differential velocities. However:

«...the principal change in the spectrum of  $\eta$  Aql at minimum of light consists in a widening of many of the lines on the violet side [outward motion], thus producing a lack of symmetry, ... ».

A modification of standard curve-of-growth technique, using residual intensities in line cores, was employed. The results are similar to those of WALRAVEN for  $\delta$  Cep as regards temperature and electron pressure variations.

The authors derived the (Lagrangian) variation of density following a particle finding the density to be some 4 times greater at the phase of maximum light and maximum outward velocity than at minimum light. This result could be very significant as it is the only available result of its kind.

The authors introduce the derived velocity and density amplitudes into the Rankine-Hugoniot equation for the isothermal case and state that the density amplitude is smaller by a factor 10 than is compatible with the velocity amplitude ( $\Delta v = 10 \times$  velocity of sound). The authors justify the application of the isothermal R-H equation through noting that 1) the temperature variation is small relative to the density variation and 2) the atmospheric motion appears to have the form of a running wave since maximum density occurs at the time of maximum outward velocity.

Through the application of the linearized equation of motion the authors construct a composite model with a high temperature upper layer and show that the discrepancy can be eliminated.

*Comments.* — The quantitative results of this paper are derived with the aid of a number of approximations and assumptions. The amplitude of the density variations must be considered uncertain by a factor 2 or 3.

Further the use of linearized hydrodynamic equations to construct a composite pulsation model has not received adequate justification.

## 7. WALRAVEN, TH.

Ref.: *Pub. Astrom. Inst. Univ. of Amsterdam*, No. 8 (1948).

Subject: A spectrophotometric study of  $\delta$  Cep based on 10 Å/mm plates.

The author did not measure velocities. His representative tracings show that blending of adjacent spectral lines is an exceedingly serious problem even at the dispersion employed. He employed the standard curve-of-growth procedure with the following results:

1) The hydrogen lines are greatly enhanced near maximum light, at which time they are too strong by a factor 10 on the basis of classical calculations using the excitation temperature derived from the metals.

2) Excitation and ionization temperatures vary by about (500 : 1000) °K during a cycle of pulsation depending on the spectral lines analysed. Electron pressure varies by a factor 10 during a cycle, reaching maximum with the temperature, *i.e.* near maximum light.

3) During increasing light, there is a pronounced relative weakening of lines of atoms and ions for which the 2-nd ionization potential is less than about 14 eV, suggesting an increase of Lyman radiation.

## 8. BRUCK, H. A. and GREEN, H. E.

Ref.: *M. N.*, **376**, 101 (1941).

Subject: Study of radial velocities of  $\delta$  Cep based on 13 Å/mm plates.

The authors quote references on earlier work concerned with differential velocities in classical cepheids. The early work was based largely on 40 Å/mm plates and was highly discordant. They note that of the 200 lines in the region  $\lambda\lambda$  4250–4650, only 10% are sufficiently free of blends to merit measuring for velocities. Their paper concludes with the following:

«Cambridge four-prism spectrograms of  $\delta$  Cep do not indicate any definite relative displacements between lines of different atmospheric level, confirming thereby earlier negative results of PETRIE and JACOBSEN. There may exist small differences between the velocity curves of groups of neutral iron and ionized titanium lines and there is evidence for the existence of relative shifts in the case of the neutral calcium line  $\lambda$  4425.6 ».

## 2. – Summaries of papers on RR Lyrae stars.

## 1. TIEFT, W. G. and H. J. SMITH.

Ref.: *Ap. J.*, **127**, 591 (1958).

Subject: A comprehensive study of *T Sert*, the first «C» type RR Lyr star to have been studied in detail.

The authors note the following characteristics of C-type variables:

- 1) Period about 6 hours.
- 2) Relatively sinusoidal light curve with an amplitude of less than 0.6 mag.

Photometric, spectrographic and radial-velocity data are presented and the following points are brought out.

1) The maximum velocity of approach lags the maximum light by 0.1 periods «in general agreement with results for the few RR Lyr stars of type *a* for which velocity curves are available.»

2) The light and color curves show real deviation from one cycle to another. The light curve fluctuates .03 mag.; the U-B color fluctuates by 0.1 mag. These two types of fluctuations appear to be uncorrelated.

3) A hump is evident on the ascending branch of the light curve. This hump is correlated with a hump on the (B-V) color curve but not the (U-B). We note that the (B-V) color is a measure of the slope of the continuous spectrum to the red of the Balmer limit while the (U-B) is sensitive to the Balmer discontinuity.

The authors estimate that the integral of the hump represents one percent of the total light emitted during a cycle. Therefore it contains about  $10^{28}$  erg



and is equivalent to the kinetic energy of a mass of  $5 \cdot 10^{25}$  g moving 20 km/s. (The amplitude of the observed velocity-curve is about 30 km/s.) If spread over the entire surface of the star, this mass contains 15 g/cm<sup>2</sup> and has an optical thickness of the order of unity in the continuum. The authors suggest that the hump may be produced by loss of thermal energy following compression of the atmosphere. Present data are not sufficient to discuss the presence or absence of transitory emission lines of the type observed during rising light in *RR Lyr*.

The authors conclude with the following remarks:

« In summary, a qualitative description of the changes in *T Sext* can be formulated as follows: A reversing layer which produces the visible spectral lines has been thrown out from the photosphere by phase 0.9. It begins to fall back around phase 0.3. Meanwhile the light of the star has increased to maximum and begins to decline, largely as a function of the behavior of the underlying photospheric layers. At phase 0.6 a new pulsation wave is emerging from the interior, brightening the star. Collision between the top of this wave and the infalling layer liberates the energy needed to produce the hump at phase 0.85, which is superimposed on the over-all increase in light coming from the lower layers; and the cycle begins to repeat. Since the slope of the rising branch is rather sensitive to the timing and energy content of the collision and since the timing, in turn, depends on two essentially independent periodicities — namely, the pulsation frequency of the star and the interval required for the upper layer to execute a rise and fall under gravitational and possibly magnetic forces as well — we are not surprised to find variations between cycles, and these most pronounced on the rising branch of the light-curve. Indeed, such a picture might provide at least a partial explanation of the common occurrence of beat periods among intrinsic variables, on the grounds that the two independent periods normally would not be perfectly synchronized.

« Clearly, this rough model, although plausible, is quite preliminary. It points up the need for high-dispersion spectra of *T Sext* and for detailed atmospheric calculations. For example, if collision of layers occurs, a thin zone of high temperature must be present, which in turn could produce emission lines, as well as the observed strongly blue color of the hump. Higher-dispersion spectra taken near phase 0.85 would be useful in looking for the emission lines. In this connection, Balmer-series emission lines have been found by STRUVE (1947, 1948) and SANFORD (1948) in *RR Lyr* at certain phases of its long period. It is of considerable interest that the emission lines are most pronounced near the long-period phase  $\psi = 0.1$ , at which phase, in turn the light curve, having lost its type *a* peak, shows the most pronounced *T Sext*-like hump on the rising branch (WALRAVEN 1949). Further, within each cycle the appearance of emission lines coincides with the light-curve hump. Because of the over-all similarity of the two stars, these observations of *RR Lyr* lend some support to the inferential picture derived above for *T Sext* ».

## 2. HARDIE, R. H.

Ref.: *Ap. J.*, **122**, 256 (1955).

Subject: A study of *RR Lyr* in three colors.

The equipment employed in this investigation is described in *Ap. J.*, **114**, 522 (1951).

This paper presents photoelectric light curves adjusted to superposition 0.3 mag. above the minimum of light. The (B-V) color curve has the same form as the light curve. The (U-B) curve shows a short-lived maximum at the midpoint of the ascending branch of the light curve. This maximum, indicating a decrease of the Balmer discontinuity, coincides with an observed weakening of the hydrogen line-absorption and with the transitory emission observed at  $H_{\alpha}$ .

### 3. SANFORD, R. F.

Ref.: *Ap. J.*, **109**, 208 (1949).

Subject: An examination of high-dispersion plates of *RR Lyr*.

The dispersion of the plates employed in this investigation (10 Å/mm, photographic; 20 Å/mm visual) is greater than that employed by STRUVE and BLAAUW (40 Å/mm), and distinct doubling of the hydrogen lines was found. Sanford interprets this doubling in the following terms:

« If the velocity variations of *RR Lyr* are caused by pulsations of its atmosphere, the behavior of the velocities from  $H$  and  $K$  and from  $H_{\alpha}$  would seem to indicate that an atmospheric wave suddenly started outward with the maximum velocity of expansion at the time of maximum light and then slowed down, reaching maximum velocity of contraction at minimum light. Since the wave persists altogether for an interval of 1.660 for both  $H$  and  $K$  and  $H_{\alpha}$ , two atmospheric layers may be forming two separate sets of absorption lines simultaneously. It is perhaps significant that the relative intensities (shortward/longward) of the two components during this stage increase with time, *i.e.*, the component belonging to maximum velocity tends to predominate in the first of the overlap interval, whereas the component accompanying the curve at minimum does so at the end of the overlap... ».

This line-doubling occurs during increasing light, as in *W Vir* stars. It is not detected in the metallic lines or the high members of the Balmer series.

Transitory emission at  $H_{\alpha}$  develops at the midpoint of the ascending branch of the light curves and lasts 0.032 periods or about 30 minutes. The fading of this emission is followed by the formation, at the same wave length, of the new, violet-displaced, absorption line of  $H_{\alpha}$ . The old, red-displaced component vanishes near phase .12 periods.

### 4. STRUVE, O. and BLAAUW, A.

Ref.: *Ap. J.*, **108**, 60 (1948).

Subject: A study of the radial velocity variations of *RR Lyr* based on 390 spectrograms of dispersion 40 Å/mm.

The authors states that:

« The purpose of this work was to investigate whether the velocity curve changes in conformity with the 41-day period of fluctuation in the character of the short-period light curve ( $P=0.567$  days) ».

The authors conclude that the behavior of the velocity curve is quite analogous to the behavior of the light curve.

1) With respect to a uniform ephemeris the times of maximum velocity are advanced and retarded by .024 periods with a period of about 41 days.

2) Emission in hydrogen occurs during the midpoint of the rising branch of the light curve. This emission occurs during those cycles when the velocity curve shows nearly its greatest retardation.

3) *RR Lyr* shows only a weak correlation between velocity amplitude and retardation of the velocity curve. However in another paper (*Ap. J.*, **109**, 215 (1959)) STRUVE and VAN HOOFF find for *XZ Cyg* an amplitude variation of some  $(15 \div 20)\%$  such that maximum amplitude occurs in cycles of retarded maximum. Most of the amplitude variation is produced by an increase of the maximum of velocity.

Therefore it appears that retardation of the maximum of light and velocity is associated with a) increased amplitude of light and velocity and b) emission at hydrogen.

### 3. - Summaries of papers on W Virginis stars.

#### 1. WALLERSTEIN, G.

Ref.: *Ap. J.*, **127**, 583 (1958).

Subject: A spectroscopic study of three Population II cepheids (two *W Vir* stars and one *RV Tau* star) based on plates of dispersions 18 Å/mm to 80 Å/mm.

Although these stars show no line splitting with the lower dispersions, the higher dispersion plates show doubling by about 50 km/s for two of the stars at their light maxima.

The *RV Tau* star M5 No. 84 shows alternating deep and shallow minima of the light curve. Wallerstein finds that, preceding the shallow minima, the maximum of outward velocity occurs 0.3 periods late and the velocity amplitude is reduced from 60 km/s (preceding deep minima) to 40 km/s. This result is in qualitative agreement with that of ABT for *U Mon*.

The writer comments that, for the *W Vir* and *RV Tau* stars, hydrogen « emission seems to have a marked maximum among stars of period 15-19 days. Emission is definitely weaker among the stars of period 20-30 days. »

The writer comments on the difference between the spectra of classical cepheids and those of Population II in the following terms:

« ... This [difference] is most apparent in the vicinity of maximum light, when the classical cepheids are close to spectral type F6 with abnormally strong hydrogen absorption lines. The Population II cepheids, on the other hand, have spectra of A5-F0 with either greatly weakened hydrogen lines or emission lines of hydrogen. The difference is so great for variables of period 15 days or more that a star can easily be separated into one of the two types using spectra of quite low dispersion... ».

## 2. ABT, H. A.

Ref.: *Ap. J.*, **122**, 72 (1955).

Subject: A study of *U Mon* (*RV Tau* variable,  $P \simeq 46$  days) based on photometric data and spectra of 11 Å/mm and 22 Å/mm dispersion.

This star is typical of *RV Tau* stars in showing alternating deep and shallow minima of the light curve. The light and velocity variations show a large degree of irregularity.

In other respects the behavior of this star is like that of *W Vir*. *I.e.* emission of hydrogen is present during increasing light. The radial velocity curve is discontinuous with a total amplitude of about 40 km/s and «just before each light-maximum a weak set of lines appears, displaced shortward with respect to the stronger lines. These lines quickly strengthen, move longward, and then fade during the next light-maximum...».

The author discusses available data relevant to the question of systematic atmosphere-streaming. Table III of his paper, here reproduced, lists integrated radial velocities of globular clusters and mean velocities of the *W Vir* and *RV Tau* stars which are contained in these clusters.

TABLE III. - *Velocities of variables in globular clusters.*

Type	<i>NGC</i> (M)	Integrated velocity (km/s) (Mayall)	Wt. $W_I$	Vari- able No.	Mean velocity (km/s) (Joy)	Wt. $W_M$	Dif. (Mean- Integ.)	Weight
W Vir	5 272 (3)	- 150	24	154	- 153	14.0	- 3	8.84
	6 218(12)	- 32	4	1	- 42	5.6	- 10	2.33
	6 254(10)	+ 73	6	2	+ 67	7.0	- 6	3.23
RV Tau	5 904 (5)	+ 45	13	42	+ 54	8.0	+ 9	4.95
	5 904 (5)	+ 45	13	84	+ 50	8.0	+ 5	4.95
	6 779(56)	- 154	5	6	- 132	7.7	+ 22	3.03
	7 089 (2)	- 3	17	11	- 4	10.8	- 2	6.60
Weighted mean	—	—	—	—	—	—	+2±6	—

It is known that the mean random velocities of non-variable stars within globular clusters are of the order of 5 km/s. Therefore, Abt's results

$$\Delta v = 2 \pm 6 \text{ km/s,}$$

is consistent with the assumption of no systematic streaming within the atmospheres of Population II variables.

Application of the curve-of-growth analysis to the line intensities indicates,



as for *W Vir*, an increase of random velocities, on the microscopic scale, with advancing phase. A random velocity of 3 km/s is characteristic of lines which have just developed and this increases to about 7 km/s as the lines shift toward the red with increasing age. The author suggests that this increase of width may be due to turbulence or dispersion of the pulsation velocity.

The author interprets the variations of line strength around maximum light as being due to variations of the continuous opacity of the atmospheric layers forming the lines. This assumption allows evaluation of the electron pressure and degree of ionization in the layers.

*Comment.* — The variations of line strength appear to be confined to the phases of line-splitting and, for the longward component, must be largely due to decreasing total mass in the layer as the shock moves upward.

### 3. ABT, H. A.

Ref.: *Ap. J., Supp.*, Ser. 1, 63 (1954).

Subject: An analysis of the spectrum variations of *W Vir* ( $P=17.3$  days) based on 10.3 Å/mm coude plates.

The author combines color and light data with integrations of the velocity curve to derive a mean photospheric radius of  $30 \cdot 10^6$  km and a pulsation semi-amplitude of about  $10 \cdot 10^6$  km.

Examination of data on variable stars in globular clusters indicates that the median observed velocities of the variables are within 10 km/s of the velocity of the center of mass of the clusters. Hence systematic streaming motions at the region of line formation are probably less than 10 km/s.

Absorption lines show duplicity around maximum light with a separation of 55 km/s. (Private communication: Within each of the two sets of lines the differential velocities are small.)

The writer describes the appearance of the absorption spectrum in the following terms:

« When they first appear just before maximum light, the shortward absorption lines are very weak but have the appearance of an F2 supergiant. The luminosity class at all phases is about Ib — certainly not II or III. The new group of lines quickly strengthens, while the longward components fade, until at phase 0.2 they have nearly reached their full strength. The spectral type becomes later until at minimum light (phase 0.62) the metallic lines indicate G6, although the G band is either absent or very weak. After minimum light the lines begin to fade, and the spectral type quickly returns to F2. Between phases 0.825 and 0.1, when both absorption components are present and weak, both indicate a spectral type of F2, although there is a large difference in excitation temperature between the two spectra ».

In a private communication dated Nov. 3 (1954) the writer states:

« ... The measures in both *W Vir* and *U Mon* indicate that the Doppler velocity (from curves of growth) increases monotonically throughout each cycle. In so far as I can tell, the line widths in *W Vir* also increase correspondingly, so that at the phase of double lines, the longward (old) components are broader than the shortward ones. In *U Mon* the lines are not so well resolved but they seem to show the longward components broader... ».

Concerning the hydrogen lines the writer states that «the behavior of the hydrogen lines is complex and shows large variations from cycle to cycle». This behavior is shown in Fig. 6 of the writer's paper and, concisely, is the following. Shortward-displaced emission lines appear at light minimum (phase 0.6) and they have vanished by phase 0.1. The emission lines are replaced by narrow absorption lines which shift toward the red. These absorption lines persist, superimposed on the emission lines, until phase 0.1 of the following cycle.

The author notes the existence of very broad shallow absorption wings around maximum light and states that they have the same velocity as the shortward metallic lines and the *H* emission lines.

#### 4. SANFORD, R. F.

Ref.: *Ap. J.*, **116**, 331 (1952).

Subject: A study of the spectrum and radial velocities of *W Vir* based on 10 Å/mm plates.

This paper presents the best existing velocity curve for *W Vir* and discusses the behavior of certain spectral lines.

Table II of the paper, reproduced hereafter, indicates that the relative intensities of the shortward (upward moving gas) and longward component change little while both are present.

TABLE II. — *Spectrograms of W Virginis with double absorption lines.*

Plate No. (Ce)	Phase (P)	Element and relative intensities: shortward to longward component ratio, <i>S:L</i>		
		Sr II	Ti II	Fe I
7102	0.94	2:3	2:3	2:1
6107	.95	2:1	2:2	2:trace
6211	.00	3:1	2:0.5	2:0
5647	.00	1:2	1:3	1:2
5617	.04	1:1	1:1	1:1
5618	0.10	1:1	1:1	1:1

Concerning the hydrogen emission, the writer states:

Emission lines of hydrogen prevail in the phase interval from light-maximum and for an interval of 0.1 *P* beyond, in which short interval *W Vir* is very little fainter than at maximum light (see Fig. 1). Each emission hydrogen line is divided into a strong shortward and relatively weak longward part by a superposed absorption line. This difference in intensity of components increases markedly from minimum to maximum of light, so that finally the longward parts are scarcely visible.

The hydrogen lines... are in both emission and absorption. The absorption lines give velocities which agree with the velocities from the absorption lines of the other elements if these lines are single, and with their longward components if they are double.

The superposed absorption components prevented satisfactory measures of the displacements of the hydrogen emission lines on the spectrograms themselves. However, microphotometer tracings which seemed fairly adequate for this purpose furnished the displacements from which the velocities in Table I were obtained. Although these velocities are liable to considerable error, they seem to give evidence of a variation as brought out in Fig. 3. The velocity trends downward from  $-70$  km/s at phase  $0.65P$  to  $-100$  km/s at phase  $0.95P$ , with little or no change thereafter until the disappearance of the emission lines at phase  $0.10P$ . The average of the emission hydrogen lines is at least  $2.3\text{\AA}$ .

The paper closes with the following interpretation of the observation:

Absorption-line velocities of *W Vir* and the relation of their changes to changes of light may be explained quantitatively by means of shock-waves. Such a shock-wave is thought of as entering the reversing layer at its bottom and passing through its successive strata at the time when the reversing layer is falling in at its maximum velocity. This imparts a high outward velocity to successive strata, beginning at the bottom. That part of the reversing layer which has not been hit by the shock wave continues to rush rapidly inward. The Doppler effect separates the absorption lines of the intruding from those of the outrushing gases. However, this would apparently require the relative intensities of the shortward to the longward components of the double lines to be weakest at the beginning and to grow stronger as the interval for double lines advances. This is not evidenced by the data in Table II...

The explanation of the emission lines of *W Vir* is not clear. If these lines are a product of the shock wave, it would seem that the emission lines begin in layers below the reversing layer, perhaps after the shock-wave has passed by this lower level. If such a mechanism is present, it might account for the continuance of the emission lines until the shock-wave has completely traversed the reversing layer.

## REFERENCES

- ABT, H. A. 1954, *Ap. J., Supp.*, Ser. 1, 63.  
 BAADE, W. 1926, *A. N.*, **228**, 359.  
 BECKER, W. 1940, *Z. f. Ap.*, **19**, 269.  
 BECKER, W. and W. STROHMEIER, 1940, *Z. f. Ap.*, **19**, 249.  
 BOTTLINGER, K. F. 1928, *A. N.*, **232**, 3.  
 CANAVAGGIA, R. 1949, *Ann. d'Astrophys.*, **12**, 21, 96.  
 CANAVAGGIA, R. 1954, *C. R. Acad. Sci.*, **238**, 2390.  
 CANAVAGGIA, R. 1955, *Ann. d'Astrophys.*, **18**, 431.  
 CANAVAGGIA, R. and J. C. PECKER, 1952a, *C. R. Acad. Sci. Paris*, **234**, 1739.  
 CANAVAGGIA, R. and J. C. PECKER 1952b, *Ann. d'Astrophys.*, **15**, 260.  
 CANAVAGGIA, R. and J. C. PECKER 1953, *Ann. d'Astrophys.*, **16**, 47.  
 COUNSON, J., P. LEDOUX et R. SIMON, 1956, *Bull. Soc. Roy. Sci. Liège*, **25**, 144.  
 COX, J. P., 1955, *Ap. J.*, **122**, 286.  
 COX, J. P., 1958, *Ap. J.*, **127**, 194.  
 COX, J. P., 1959, *Ap. J.*, **130**, 296.  
 COX, J. P., 1960, *Ap. J.*, **132**, 594.  
 COX, J. P. and C. A. WHITNEY, 1958, *Ap. J.*, **127**, 561.

- EDDINGTON, A. S., 1941, *M. N.*, **101**, 182.  
 EDDINGTON, A. S., 1942, *M. N.*, **102**, 154.  
 HARRIS, D., 1954, *Ap. J.*, **119**, 297.  
 HITOTUYANAGI, Z., 1952, *Sc. Rep. Tôhoku Univ.*, Ser. I, **36**, No. 4.  
 HITOTUYANAGI, Z. and K. VJI-IYE, 1956, *Sc. Rep. Tôhoku Univ.*, **40**, 54.  
 KUBIKOWSKI, J., 1959, *Ann. d'Ap.*, **22**, 74.  
 LAUTMAN, D. and L. SPITZER, Jr., 1956, *Ap. J.*, **123**, 363.  
 LEDOUX, P., 1941, *Ap. J.*, **94**, 537.  
 LEDOUX, P. and J. GRANDJEAN, 1954, *Ann. d'Astrophys.*, **17**, 161.  
 LEDOUX, P. and J. GRANDJEAN, 1955, *Bull. Acad. Roy. Belg. C. Sci.*, Ser. V, **41**, 1010.  
 LEDOUX, P. and TH. WALRAVEN, 1958, « *Variable Stars* », *Hdb. der Physik*, ed. S. FLÜGGE, vol. **51**.  
 MARSHAK, R. E., 1958, *Physics of Fluids*, **1**, 24.  
 OKE, J. B. (private communication). OKE, J. B. and S. J. BONSAK, 1960, *Ap. J.*, **132**, 417.  
 OPOLSKI, A. and J. KRANĚCKA, 1956, *Trav. Soc. Sc. Lett. Wrocław*, Ser. B, No. 81.  
 PETSCHKE, H. E. and S. BYRON, 1957, *Ann. of Phys.*, **1**, 270.  
 SACHS, R. G., 1946, *Phys. Rev.*, **64**, 514.  
 SCHATZMAN, E., 1956, *Ann. d'Astrophys.*, **19**, 51.  
 SCHWARZSCHILD, M. and R. HÄRM, 1959, *Ap. J.*, **129**, 637.  
 SCHWARZSCHILD, M., B. SCHWARZSCHILD and W. S. ADAMS, 1948, *Ap. J.*, **108**, 207.  
 SEN, H. K. and A. W. GUESS, 1957, *Phys. Rev.*, **103**, 560.  
 STEBBINS, J., 1945, *Ap. J.*, **101**, 47.  
 STEBBINS, J., 1953, *P.A.S.P.*, **65**, 118.  
 STEBBINS J., G. E. KRON and J. L. SMITH, 1952, *Ap. J.*, **115**, 292.  
 TESKE, R. G., 1960, Ph. D. Dissertation, Harvard University.  
 VAN HOOF, A., 1943, *Kon. Vlaam Akad. v. Wetensch.*, **5**, no. 12.  
 WESSELINK, A. F., 1946, *B.A.N.*, **10**, 91, 256, 330.  
 WHITNEY, C. A., 1955a, *Ann. d'Ap.*, **18**, 375.  
 WHITNEY, C. A., 1955b, *Ap. J.*, **122**, 385.  
 WHITNEY, C. A., 1956, *Ann. d'Ap.*, **19**, 34, 142.  
 ZUEVAKIN, S. A., 1960, cf. especially *Astron. Journ. USSR*, **36**, 269, 394, 996, where his latest ideas and results on the subject are summarized and many references to his earlier work are given.



## PART III.

### Spherically-Symmetric Motions in Stellar Atmospheres.

#### A. - Pulsating Variable Stars.

#### Discussion.

*Chairman:* E. SCHATZMAN and R. N. THOMAS

— E. BÖHM-VITENSE:

If one considers a static model stellar atmosphere, characterized by  $T_{\text{eff}} \sim 5000^\circ$  and  $g \sim 30 \text{ cm/s}^2$ , which are the values just discussed for some of the cepheids, he finds that the radiative acceleration  $-g_r = -\kappa_0 F/c$  can at some depths exceed the gravitational acceleration  $-g = GM/R^2$ .  $\kappa$  is the continuous absorption coefficient, and  $F$  is the radiative flux. This situation occurs at a depth where  $T \sim 10000^\circ$ . Here, the net acceleration will be outward, and the gas pressure will decrease inward; in deeper layers,  $\kappa$  decreases and the effect reverses. Can this effect be of importance when considering cepheids?

— C. A. WHITNEY:

If you consider the fact that convective transport may reduce the radiative flux, will the radiative acceleration still predominate?

— E. BÖHM-VITENSE:

Yes. These models are just in the temperature-gravity region where the instability cannot cause convection because of radiative exchange between convective cells.

— W. B. THOMPSON:

WHITNEY must have taken this effect into account in discussing the non-static case. For if you have motions, then the effect of radiation will be to transform the hydrodynamic shock into a radiative shock, in which there is a transition from opaque to transparent regions occurring across the shock front.

— C. A. WHITNEY:

Mrs. BÖHM-VITENSE considers the  $T=10^4$  level; while my calculations apply to a much higher level, where the hydrostatic model predicts essentially neutral hydrogen.

— R. LÜST:

Two other questions. 1) What would be the effect of shock waves on the light-curve; 2) How does this picture of shock waves compare with Lautman's calculations?

— C. A. WHITNEY:

LAUTMAN used the non-dimensional equations, but assumed small amplitudes, so that in a sense the integrations were linear. He used the same lower boundary condition as I did, a piston. However, he used a frequency considerably lower than I did, and therefore more appropriate to cepheids.

At the top he used as boundary condition the relation between velocity and velocity gradient which would hold in the linearized case in the absence of downward-running waves. I am not sure that his conditions are physically significant because the situation is actually non-linear.

Another point to notice is that he also did integrations assuming perfect reflection at some particle, and found that the character of the solutions was significantly altered by this change of boundary condition.

Now to the other point, how does the velocity curve with a shock affect the light curve? Since the light curves resemble closely the velocity curves, the feeling has been that the motions in the atmosphere produce the light variations.

However, if there is only a weak shock or a progressive wave, the rate of production of thermal energy by compression is a very small fraction of the stellar flux. Therefore the atmosphere acts only as a filter to the radiation, passing through it.

On the other hand, the rate of production of thermal energy can be significant in the presence of strong shocks, and could produce a modification of the light curve over a small phase interval.

— A. UNDERHILL:

I would draw your attention to the  $\beta$  *Cephei* stars, which have pulsation periods near 6 hours, and atmospheric temperature  $((20 : 25) 10^3)$ . The light curves have small amplitude ( $< 0^m.05$ ), while the range in velocity exceeds 100 km/s. One observes that the velocity increases rapidly, then there occurs a discontinuity as the velocity decreases. On spectra of the highest dispersion, you may see double, and occasionally triple, sets of lines during the very short period when the velocity decreases. The period of one star, *BW Vulpeculae*, has been increasing over the (25 : 30) years it has been observed. ODGERS and KUSHWAHA have discussed the observations of this star in terms of an isothermal shock.

— A. J. DEUTSCH:

The discontinuity in the velocity curve is really an astonishing thing. In a 5 minute period, one sees the velocity indicated by the hydrogen Balmer lines change, apparently discontinuously, by nearly 150 km/s. It is the most striking phenomenon encountered in any of the pulsating variables.

— A. UNDERHILL:

Depending upon your temporal and spectral resolution you also get two and sometimes three sets of Balmer lines. You get the feeling that you see different volumes of gas at the same time. The line that is moving at  $\pm 100$  km/s gets weaker, and you do not see much of it—but while you can still see it, you find another one quite strong, but moving outward,  $-50$  km/s. Also note that  $H_{\alpha}$  goes into emission for a very short time, during the cycle.

— L. DAVIS:

Can one assume that the two or three velocities seen simultaneously refer to patches at different places on the star, or must he assume that they represent spherically-symmetric velocities?

— A. UNDERHILL:

I don't know whether one sees patches or several spherically-symmetric shocks at once, but the spectral variations one sees for shell-stars and or supergiants make one ask this question. It is a question of the relative life-times of shocks in these atmospheres, and the path-lengths through which one can observe at any moment. I suspect that one observes several shocks at once; otherwise, he would not observe some of the sudden doublings and widenings.

— P. LEDOUX:

I would like to make a general comment on these  $\beta$  Cephei stars. In many of these stars, the line profiles change in the course of the cycle. HUANG has noted that this effect does not affect the equivalent width but consists simply in a widening of the line. This favors the idea that we are looking at different parts of the star surface and not at superposed layers.

On the other hand, many of these stars have two extremely close periods. Despite the fact that these periods may vary in the long run, they are very stable in the sense that the difference between them remains the same for many cycles, giving rise to a very regular beat phenomenon so that the amplitude is modulated with a period which may be 30 to 200 times the short period. This again makes it very difficult to interpret these stars in terms of purely radial oscillations. As far as I am concerned, I fail to see how you can get two very close periodicities on this basis without very artificial assumptions

leading to near-commensurability of the periods of two radial modes and in-voking coupling.

The easiest way out of this is to apply non-radial oscillations in the presence of rotation or a magnetic field. For instance, let us consider the mode represented by a spherical harmonic of order two. In absence of rotation (or magnetic field), we get only one period. But if rotation is present the degeneracy of the frequencies disappears and we can get two periods that are very close to each other. On that picture, part of the star would be moving out while the rest is moving in and this could at least qualitatively account for the changes in the line-profiles, which was the only effect known when I first discussed this problem.

I don't know how the strong discontinuity discussed above in the case of *BW Vul* could be explained on this basis. However, I would like to note that the velocity curve which has been shown suggests a very strong non-linearity, although, if interpreted in terms of radial pulsations, the relative amplitude in these stars,  $\delta R/R$ , is very small, of the order of 0.01; *i.e.* appreciably smaller than in the classical cepheids, even those that exhibit the smoothest behavior.

— H. PETSCHER:

It was mentioned that the period had changes in the last 25 years. On the basis of a simple radial mode of an acoustic oscillation how do you explain a change in the period?

— E. SCHATZMAN:

The theories which have been developed on the pulsation show that the period is an extremum property of the whole star, and if the period changes it means that something in the whole star is changing. In the case of the cepheids, for example, some of them are known for (150-180) years—with not quite one second change in period in that length of time, which shows that during the last 180 years the star has not changed its structure by any appreciable amount.

— P. LEDOUX:

The main difference between the ordinary cepheids and the  $\beta$  *Cephei* stars is that the latter are B stars, which we believe evolve very rapidly to the right of the main sequence, with increasing radii and decreasing densities. STRUVE has shown that the decrease in density necessary to account for the increase in period (cf. formula (19) in the text) is compatible with the normal theory of stellar evolution.

— E. M. BURBIDGE:

What would you say the period should be for stars of this type?



— P. LEDOUX:

The observed  $Q$ -value is about 0.025 and it is difficult to account for it on the basis of purely radial oscillations unless we adopt a model with an extremely high mean value of the effective polytropic index through the star. This does not seem likely according to the usual views on stellar structure in the relevant part of the Hertzsprung-Russell diagram.

The first  $p$ -mode of a non-radial oscillation corresponding to a spherical harmonic of degree two would certainly be more favorable in that respect.

However, there may still be doubts on the correct models and masses for these stars so that it is difficult at this stage to reach definite conclusions.

— S. S. HUANG:

The most striking observational result is that the periods of the *Canis Majoris* stars either remain constant or increase with time—no single example of a decrease has been found. Therefore, we attribute the increase to stellar evolution. As LEDOUX pointed out, a massive star evolves rapidly. When it departs from the main sequence after exhausting the hydrogen in its core, its radius increases, hence its density decreases. Therefore, its period will increase according to the  $P\sqrt{\bar{\rho}} = \text{constant}$  relation.

— E. SPIEGEL:

The so-called van Hoof effect consists of the existence of phase differences in plots of velocity *vs.* time, for lines from different elements. Are there any differential velocity effects observed between elements for these  $\beta$  *Cephei* stars?

— A. J. DEUTSCH:

Such velocity differences do exist in this star, measured by these phase differences.

— P. LEDOUX:

There is some confusion here. The van Hoof effect consists in the fact that the doubling of the lines does not occur at the same time for different lines. The doubling occurs progressively in those lines whose origin is higher and higher in the atmosphere, exactly as if a discontinuity were indeed moving outward.

— *Ed. Note:*

At this point in the discussion, there arose questions on the source of the pulsational instability. The discussion for the balance of the morning session was long and confused. For this reason, LEDOUX has revised and very con-

siderably amplified the treatment of pulsational instability in the text of the introductory summary, bearing in mind the source of difficulties encountered during the discussion.

During the afternoon recess, a smaller group held informal discussion, and an unsuccessful attempt was made by CLAUSER and others to present some consensus of opinion on an « aerodynamic » look at the problem of the instability mechanism maintaining the oscillations. Various simplified thermodynamic systems were outlined in an attempt to construct a system schematizing a star. The aim was to clarify the origin of cepheid instability in the simplest physical terms. Again, the result of the discussion was essentially confusion. Consequently, WHITNEY has prepared the following outline of a model, essentially due to EDDINGTON, as a preferable substitute for any attempt to provide an edited coherent account of the actual discussions of this topic.

— *Model presented by Whitney:*

Consider a plane-parallel homogeneous slab of gas whose lower boundary is stationary. Let the slab be confined above by a transparent piston whose height varies sinusoidally with an amplitude small relative to the slab thickness. If the frequency of the piston is kept very low relative to the resonant frequencies of the slab, hydrostatic equilibrium will be maintained. Let  $P(t) = \bar{P} + \hat{P}(t)$  be the pressure within the slab and let  $V(t) = \bar{V} + \hat{V}(t)$  be the volume of a unit column within the slab; bars denote mean values. (Note: Put  $V(t)$  as just the piston height.) Write

$$(1) \quad \partial V(t) = A \sin \omega t.$$

Let there be an energy flux  $F_i(t)$  upward through the lower surface of the slab with

$$(2) \quad F_i(t) = F_i[1 + \varepsilon_i \sin(\omega t + \varphi)].$$

By analogy with the nature of the radiative transfer process at great depths within a star, we should consider this flux to be carried through the slab by thermal conduction, with a temperature-dependent coefficient of conduction. The variations of  $F_i(t)$  produce a thermal wave which propagates up through the slab. The amplitude and phase of the flux at the top of the slab are determined essentially by the heat capacity of the slab and the law of temperature-dependence of the coefficient of conduction.

However, at this stage we allow our analogy with the stellar case to be weakened in order to simplify the algebra. We neglect conduction and assume energy transfer to be purely radiative. Further, we require that the gas within

the slab be optically thin so that the absorption takes place uniformly throughout the slab. The net rate of absorption per gram will then be determined by  $F$ , and the opacity of the gas. We express this absorbed energy as  $F(t) = F \sin(\omega t + \varphi)$ .

If  $F(t) = 0$ , the gas pressure  $P(t)$  will be related to the volume of the box,  $V(t)$ , through the adiabatic law and we may write for the small variations,

$$\delta P(t) = -\gamma \delta V(t) \frac{\bar{P}}{\bar{V}},$$

where  $\delta V(t)$  is given by equation (1).

The total work done on the piston during one cycle,  $W$ , is then

$$W = \oint \delta P \frac{d}{dt} \delta V dt = -\gamma \frac{\bar{P}}{\bar{V}} \omega A^2 \int_0^{2\pi} \sin \omega t \cos \omega t dt = 0,$$

and vanishes.

When  $F(t)$  is non-zero, but its cyclic integral vanishes, *i.e.*

$$\int_t^{t+(2\pi/\omega)} F(t) dt = 0,$$

the energy equation in linearized form is

$$\frac{\gamma}{\gamma-1} \bar{P} \frac{d}{dt} \delta V + \frac{\bar{V}}{\gamma-1} \frac{d}{dt} \delta P = F(t).$$

This equation has the following integral

$$\delta P(t) = \frac{\gamma-1}{\bar{V}} Q(t) - \gamma \delta V(t) \frac{\bar{P}}{\bar{V}},$$

where

$$Q(t) \equiv \int_0^t F(t) dt = \int_{2\pi n/\omega}^t F(t) dt.$$

The pressure variation now contains a term proportional to  $Q(t)$ , the heat absorbed since the commencement of the current cycle, *i.e.* since  $t = 2\pi n/\omega$ .

Inserting  $F(t) = F \sin(\omega t + \varphi)$  leads to

$$\delta P(t) = -\frac{\gamma-1}{\bar{V}} \frac{F}{\omega} \cos(\omega t + \varphi) - \gamma \frac{\bar{P}}{\bar{V}} A \sin \omega t.$$

Inserting this into the work integral gives

$$W = -\frac{\gamma-1}{V} FA \int_0^{2\pi} \cos(\omega t + \varphi) \cos \omega t \, dt = -\pi FA \frac{\gamma-1}{\omega V} \cos \varphi.$$

Noting that  $\varphi$  is the angle by which the absorption rate leads the volume change, we see that the implications of this relation are the following: If  $W$ , the net work done on the piston is positive, the system is unstable in the sense that there is a net transfer of energy from the radiation field into the mechanical system driving the piston.

For various values of  $\varphi$  we have

$$W = 0 \quad \text{when} \quad \varphi = \pi/2, 3\pi/2,$$

$$W > 0 \quad \text{when} \quad \pi/2 < \varphi < 3\pi/2,$$

$$W < 0 \quad \text{when} \quad 3\pi/2 < \varphi < \pi/2.$$

The major weakness of the analogy between the system discussed above and the cepheid envelope is in the assumption that the slab is optically thin. A further weakness lies in the assumed boundary conditions of a stationary lower boundary and a driven piston above.

In reviewing the discussion of this subject during the Symposium, it is clear that the major source of confusion was the weakness of the analogy between this simple system and the cepheid envelope.

(*Ed. Note:* Several questions were raised which are appropriate to the model presented; we reproduce these, and their answers; then Pecker's question on asymmetry of line profiles marks the turn of the discussion from this topic of instability source.)

— H. PETSCHER:

In the model described, the flux is absorbed instantaneously. But in the stellar case, the radiation takes a time of the order of a million years to get from the center to the region where you want to absorb it.

— P. LEDOUX:

Energy generated at the center will take a long time to reach the surface. But we consider a star that has reached a steady state in which the flux at any point is determined by the local temperature gradient. In the same way, the disturbance of flux at a given point at a given instant is determined entirely by the local perturbation of the opacity, of the radiating power per unit surface, and by the local change of the temperature gradient. Effectively, the



problem can be described in terms of a heat conduction coefficient. We may leave out of consideration the excess heat generated close to the center of the star. This simply provides another local contribution to the instability, which is automatically taken into account in the integral expressing the coefficient of vibrational instability (cf. the text). This excess energy generation does not determine the excess flux. The point is that in a star, the energy generation per second—or the total rate of energy radiation—is only a very small fraction of the internal energy (here  $10^{-12}$  to  $10^{-14}$ ). So we have an enormous energy reserve, and the flux will adapt itself at each instant to the local conditions.

— H. PETSCHER:

Yes, but the effect you are describing is an effect of varying heat conduction coefficient. Can you show why more energy is stopped where the temperature is high due to the wave. I am willing to let you vary the opacity any way you like—I still do not understand the heat flux mechanism.

— P. LEDOUX:

What you need in the piston analogy is that, while you compress the gas, its opacity should increase, subtracting from the flux some energy which is transformed into thermal motion and an excess pressure.

— A. J. DEUTSCH:

Am I to infer now that the question of the phase lag is now understood, and that it no longer constitutes the problem it once did?

— P. LEDOUX:

I would like to emphasize that what I have done this morning is to try to summarize the present state of two of the fundamental problems associated with the interpretation of the cepheids: 1) what is the origin of the instability? 2) if this instability is due to the ionization of an abundant element in the external layers, does it, at the same time, produce a phase lag of the order of that observed?

The work of ZHAVAKIN and COX confirms that the second ionization of He has a large destabilizing influence; but whether it can make the whole star vibrationally unstable has to be checked by detailed computations. A phase-shift also arises, but its value is certainly not as critical for instability as was once suggested by EDDINGTON. Again, in the case of the cepheids, only detailed computations on realistic models could show whether this phase-shift is similar to the one observed. My own feeling is that the problem is still far from being definitely settled but the present line of approach is promising.

Are there other lines of approach? As far as the instability is concerned, we cannot quite be sure until we possess a reasonable model for the internal structure of these stars; and the possibility of hard self excited oscillations should not be discarded completely. As far as the phase-shift is concerned, the anharmonicity may contribute to it; and furthermore, the exact interaction between the oscillations of the interior and those of the atmosphere—which may be considered, in part, as driven by the variable flux issuing from the interior—has not received a lot of attention.

— A. UNDERHILL:

I just want to remark that the move toward He II as the driving force of cepheid variation makes me very happy; because in the early type stars, hot O's, and B's, there is ample evidence that small fluctuations of light and radial velocity take place, and in some ways can be qualitatively compared to cepheid variations. But you know perfectly well that in these atmospheres there is no hope of hydrogen convection arising—but there is of helium. You would like to have the same thing work for both types of stars—so qualitatively I am very pleased to hear this result on He II.

— R. LÜST:

I would like just to add to the remarks of LEDOUX that also BAKER and KIPPENHAHN are making similar calculations in Munich. But it is too early to say something definite about the results. They try to fit an adiabatic interior to a non-adiabatic shell.

— E. BÖHM-VITENSE:

I should like to make a remark which may confuse the matter again. But I would like to point out that cepheids in the H-R diagram appear just at the line where the stars change from having a hydrogen convection zone to where they do not. Since this transition is rather abrupt, it seems possible that during the course of pulsation the star may change from a state with active convection to a state where there is no convective energy transport. And so perhaps the driving mechanism may also be correlated to switching on the convection and turning it off again.

— J.-C. PECKER:

I would just come back on the empirical determinations of velocity field. There are essentially three ways of getting information on differential velocities within the atmosphere. Let us consider (Fig. 1)—the different layers of the atmosphere. They could pulsate or as in *a* (standing wave) or as in *b* (progressive wave)—the outer layers being above.

1) LEDOUX has described how we can obtain radii—first from consideration of the radial velocity curve—and then from the consideration of the photometric curves—coming from the combination of data concerning luminosity and temperature. Now if Oke's results (which seem very conclusive) are taken into consideration, it means essentially that the radii determined from luminosity curve and from velocity are in agreement—in other words the radial velocity curve is the same from different ways to get to it. This conclusion is not necessarily in favor of the type of description of Fig. 1-*a*, because it can be very well the case that during all the processes of the pulsation we see the same material layer of Fig. 1-*b*—the interpretation of the difference between the two radii could have been that we see at different moments different layers, as schematically indicated by the crosses and dotted lines on Fig. 1-*b*. And thus, the new agreement obtained between the two radii does not particularly favor the standing wave more than the progressive wave.

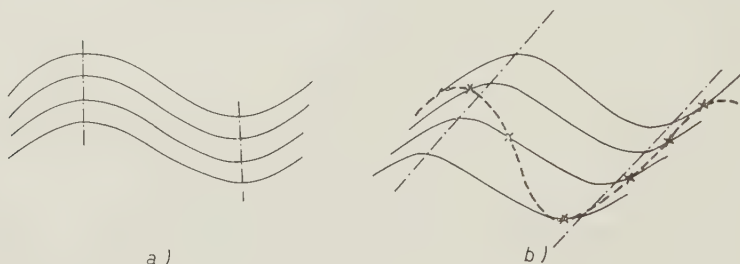


Fig. 1.

2) The second evidence in favor of rate of variation of velocity with depth has been given by WHITNEY when he presented the time variation of the different radial velocities from line to line curves around which the points were quite scattered. I do not want to comment on this: it is only obvious that experimentalists should look at the question with a greater accuracy than previously done.

3) There is a possible third way of inference of the variation of radial velocity with depth—a way which is a very difficult one indeed and which requires a detailed theory of the atmosphere, but which could anyway be used:—In a cepheid—in a variable star—the lines are asymmetrical and the asymmetry can arise in two ways. The main one is that we integrate over the disk of the star. In addition to this, a gradient of velocity in the atmosphere can influence the asymmetry. The study of the asymmetry of the lines, I think, should be taken, as a very difficult way but as a possible one, to get to those differential velocity effects. I do not know of any recent work on interpretations of asymmetries in any completely satisfactory way.

— C. A. WHITNEY:

The observational data that you suggested are very difficult to use because the purity of the spectrum is not what we would like. TESKE, from Harvard, has been looking in a theoretical sense at constructing profiles in an atmosphere with a velocity gradient. The work is not completed yet, but it does give some hope if we can just get better spectra. The work so far has been done with the LTE approximation, since the mathematics even in this case is fairly involved.

— W. H. MCCREA:

I wish to put forward the suggestion that cepheid pulsation may be essentially a *resonance phenomenon*. This would mean that every star in some general category would have two characteristic times associated with it, but that a star in this category would be a pulsating star only if these two times are equal or commensurate. If this suggestion is valid, it is natural to expect that one of the characteristic times would be associated with the main part of the interior; while the other time would be associated with its outer part, or envelope. In this context, it should be remembered that pulsating stars do apparently possess extended atmospheres that might have larger characteristic times for associated phenomena than the atmospheres of, say, main-sequence stars.

The following properties may support the suggestion:

a) We have the fact that the pulsation phenomenon is restricted to very narrow regions in the H-R diagram. SCHATZMAN has reminded us that no known peculiarity in nuclear processes of energy generation accounts for instability in these regions. In that case, the occurrence of instabilities is indeed characteristic of a resonance phenomenon.

b) We have also been reminded by LEDOUX that there is in fact in the observations some evidence for the presence of two periods with the occurrence of beats, and not just one simple period, in some cepheid phenomena. This seems to be rather direct support of the suggestion.

c) Again, it has been pointed out that there are at least three chief types of pulsating star. A possibility on the basis of the present suggestion might be that there are stars in which the two characteristic times are equal, stars in which one is twice the other, and stars in which some other simple commensurability occurs.

d) If cepheid pulsation is a simple periodic phenomenon then, as has often been said, it is very hard to see why the oscillations are excited in some stars and not in others. According to the present suggestion, simple periodic oscillations may occur for any star. But normally they would be an exceed-



ingly feeble phenomenon. I believe, however, that there is some observational evidence for such weak periodic phenomena in some stars other than cepheids. While any star may show these weak periodic effects, my suggestion is that it will show strong periodic effects only when a resonance occurs.

e) I think the suggestion is not out of accord with much of today's discussion. From various points of view, we have had the concept of an oscillation of the interior of a star being linked with phenomena in the other regions. My suggestion would change nothing in all this except to require that these latter phenomena should have a characteristic time, and that the oscillation would lead to a « pulsation » only if this time is commensurate with this oscillation period. Further, my suggestion does not alter the need for a way of « driving » the pulsation as has been discussed.

f) A very tentative quantitative test may be noted. The characteristic time  $t_1$  for an oscillation of the main part of a star is of the order  $R/a_1$ , where  $R$  is the radius and  $a_1$  is sound-speed in the interior. A characteristic time  $t_0$  associated with an envelope could be  $H/a_0$ , when  $H$  is the depth of the envelope and  $a_0$  is sound-speed in this region. Thus, roughly,

$$t_1/t_0 = (R/H)(a_0/a_1) = (R/H)(T_0/T_1)^{\frac{1}{2}},$$

where  $T_0$ ,  $T_1$  are typical temperatures for the envelope and for the interior. Now suppose very provisionally that the depth of the envelope is fixed by the level at which helium becomes ionized, in conformity with some of the indications of the discussion. This would require  $T_0$  to be of the order of  $10^5$  degrees, and we know that  $T_1$  is of the order of  $10^7$  degrees. Thus  $(T_0/T_1)^{\frac{1}{2}}$  would be of the order of 0.1. For resonance to be possible,  $t_1/t_0$  would have then a value about unity. Then we should have  $H/R \sim 0.1$ . This would require the critical level for the ionization of helium to be at about 0.1  $R$ , which is not unreasonable. It seems, therefore, that the suggestion is worth pursuing.

— E. SPIEGEL:

I would like to suggest a physical reason for the possibility of a two-period situation as McCREA has suggested. Suppose that this outer zone that he mentioned—it need not be the entire outer zone—were convectively unstable and there were some rotation. We expect that at the low Prandtl numbers characteristic of the stellar atmospheres (the Prandtl number is the kinematic viscosity divided by thermal conductivity), the instability would arise as over-stability; *i.e.* a periodic oscillation. This has been studied in the incompressible situation by CHANDRASEKHAR. This over-stable layer has a natural frequency, and this provides a possibility for mechanical driving of pulsations

of the star. This would be, of course, a weak input, but if there were a resonance of the sort mentioned by McCREA, you might expect it to be potentially a mechanism for driving pulsations. I really think of this in connection with the  $\beta$  Cepheid stars—for various reasons I will not go into. The difficulty is that it is hard to evaluate what the frequency of the over-stability is because the calculation has been done only for a plane-parallel case in an incompressible medium, and moreover, it has only been done in the stability situation. In the stars, we have clearly a highly unstable situation in which the stability period may not be relevant, but the qualitative calculations have shown that it is not really impossible to expect this kind of thing.

— P. LEDOUX:

Although very interesting in itself, the idea of a resonance in a continuous hydrodynamical system between two parts of the system seems difficult to apply unless one has good reasons to treat these two parts as practically independent. Eigenperiods are only defined by boundary conditions and, if the boundary condition at the common boundary between the two regions contemplated is the continuity of the displacement and the pressure, the two regions cease to be independent units. Furthermore, resonance does not free us from finding a source of mechanical work capable of amplifying the small motion with which we start.

— W. B. THOMPSON:

The question has been raised as to the influence of radiation when it is included in the discussion of the shock. There are laboratory situations where you try to produce a hot shock; and when you do, you find a precursor, which has been associated with radiation running ahead of the shock. The precursor seems to play a vital role in the whole role in the laboratory. Whether there is an analogy in the stellar atmosphere I do not know.

— R. N. THOMAS:

This question, indeed the whole question of transient affects, is extremely interesting, if one starts talking about hydrogen and helium lines going into emission. Some of us are reasonably convinced that we can now do a good job on the non-LTE calculation of calcium and hydrogen lines in the time-steady-state situation. As far I know, this approach has not been applied to discuss the problem of emission lines in the cepheid atmospheres, but I am confident that it will have to be, in time. However, if it should turn out that transient effects occur so rapidly that the steady-state computations become invalid, we have a more nasty species of non-LTE calculations to make. I am surprised that this question of relaxation times has not been of wider concern here.

— P. LEDOUX:

There is a question on which the comments of the aerodynamicists would be very welcome, namely: how do we pass exactly from the simple picture of a linear standing oscillation of the whole star to that of a finite oscillation with running waves or shock-waves at least in the external layers?

Let us suppose first that we have a perfectly reflecting boundary and that the oscillation remains adiabatic right through to the surface but that, by some means, we increase progressively the amplitude. Will the oscillation remain a purely standing wave even when the velocity associated with the periodic displacement becomes larger than the local velocity of sound of some region of the star (usually this happens first close to the surface)? For instance, we can find such solutions for the adiabatic oscillations of the homogeneous compressible model. Are they meaningful?

On the other hand, if we have no sharp boundary (surrounding medium) or if we have some dissipation in the external layers, the oscillation in the external part of the star, even in the linear approximation, will acquire a more or less important progressive part. It is this part which gives rise to shock conditions as the amplitude increases? Where and when will this happen?

— H. PETSCHER:

I would think that one could answer this in terms of the time it takes a pressure pulse to steepen to form a shock-wave. If one makes the piston assumption WHITNEY has used, one can compare the distance it takes the pulse to steepen with the scale-height of the atmosphere. If the distance is less, you certainly get shock-waves.

— *Ed. Note:*

From this point on, the discussion turned to what one could say about the effect of an atmospheric density gradient on the steepening process—the steepening in an homogeneous atmosphere following the usual Riemann arguments. MINNAERT emphasized that in the astronomical case, the wave-length was very large compared to atmospheric extent, so that probably the question of the effect of atmospheric density gradient was all-important. Since the problem of this density-gradient formed the subject for a future session, further discussion was deferred. Because this was the turn of the discussion, these records have been altered from their chronological order to make that session the next reported.

## PART III.

### Spherically-Symmetric Motions in Stellar Atmospheres.

#### B. - The Propagation of a Shock-Wave in an Atmosphere of Varying Density.

##### Summary-Introduction.

E. SCHATZMAN (\*)

*Institut d'Astrophysique - Paris*

##### 1. - Introduction.

There is astrophysical evidence for the existence of shocks propagating in regions of variable density. We have therefore the choice of discussing first the physics of shocks in a variable density atmosphere, or the astrophysical phenomena. Following Kaplan's preliminary report, we shall describe first the astrophysical facts, for the reader to be able to understand the connection of the physics with the astrophysics.

1.1. *Novae and Supernovae.* - There is a widespread belief that *Novae* and *Supernovae* outbursts are due to the appearance at the surface of a star of a shock front somewhere inside (LEBEDINSKY (1946), SCHATZMAN (1946*a, b*), ROSSELAND (1946), GUREVITCH LEBEDINSKY (1947)).

Several questions arise, concerning the production of shocks in novae:

- (i) Nature of the instability initiating the shock.
- (ii) Energy sources of the shock. Has the shock a nuclear origin or is it produced by some other physical process?
- (iii) Propagation of the shock in layers of decreasing density. The methods used for describing that process will be given later in this paper.

It is not necessary to recall here Milne's picture of the nova phenomena

(\*) A considerable help in preparing this report was the preliminary report of Dr. S. A. KAPLAN from Lvov Observatory.

Ed. Note: Last minute circumstances prohibited Dr. Kaplan's attendance at the Symposium and Dr. Schatzman kindly undertook to prepare and present this report.



as the sudden collapse of an unstable star, with liberation of gravitational energy, though it can be connected with some of the modern pictures of supernovae. Biermann's picture (1939) of a sudden release of recombination energy, with formation of a convective zone, has been objected to by LEDOUX, because it cannot be a sudden phenomenon.

SCHATZMAN (1958) has shown that vibrational instability cannot lead to any acceptable picture of the recurrence of a nova, as the time scale of the recurrence would then be of the same order of magnitude as the Helmholtz-Kelvin time scale of the contraction. He has shown that vibrational instability in the presence of a resonance-induced oscillation of finite amplitude could lead to a reasonable theory of the recurrence.

It is a great temptation to suppose that the shock is initiated by a detonation wave, the exploding fuel being some convenient nuclear species. However, for most kinds of nuclear fuels, it can be shown (SCHATZMAN, 1951) that the thickness of a detonation wave is much larger than the radius of the star, unless the cross-section of the energy producing nuclear reaction is exceptionally large and the abundance of the nuclear fuel great enough. The conclusion is that the energy appearing in the novae phenomenon, about  $10^{45}$  ergs, has to be liberated in a time shorter than the half period of oscillation of the star (about  $10^4$  s), in a non-linear phenomenon, the surface appearance of the shock being only due to the propagation of a wave in regions of decreasing density.

Spectroscopic observation of novae shows the existence of systems of lines, with different radial velocities. It seems that the envelope which has been ejected is made of several shells catching up with each other. (See, for example, the data collected by C. PAYNE-GAPOSHKIN (1957), and the well-known book of VORONTZOV-VELYAMINOV: *Gaseous Nebulae and Novae* (1948)).

There is a large variety of novae, and it is not the place here to classify them. However, it should be mentioned that it is unlikely that one process only is producing the novae outbursts. Several nuclear reactions, depending upon the range of density, temperature and chemical abundances can lead to explosive processes.

The problem of supernovae is likely to be different, in the sense that the whole star seems to be blown apart by the explosion. The total amount of energy liberated is of the order of  $10^{49}$  erg (the energy at rest of the whole sun is  $2 \cdot 10^{54}$  erg, and its gravitational energy is of the order of  $4 \cdot 10^{50}$  erg). Evolution of a contracting star can eventually lead to nuclear reactions which make the star dynamically unstable. A collapse, with a large temperature and density increase, can favor a large variety of nuclear reactions which have been investigated by BURBIDGE, BURBIDGE, HOYLE and FOWLER (1957). However, the hydrodynamics of the collapse and the generation of the shock-wave have not been investigated except by COLGATE (1959).

1'2. *Cepheids*. — The problem of shocks in a variable density atmosphere is now considered as a standard problem of *Cepheids*, and has been discussed by WHITNEY in his introductory report.

1'3. *Solar chromosphere and corona*. — According to BIERMANN (1948), SCHWARZSCHILD (1948), SCHATZMAN (1949*b*), the heating of the solar chromosphere is due to energy dissipation of compression waves, created by granulation. THOMAS (1948) has suggested that the heating is due to the dissipation of the kinetic energy of the spicules.

The production of sound waves by turbulence and their propagation out of the turbulent regions is a well-known observed fact (cf. the summary *Aspects of the Turbulence Problem* by H. LIEPMANN, 1952).

Therefore, it can be considered as certain that compression waves, produced in the hydrogen convective zone, do propagate outside, towards the chromosphere and corona, though no astrophysical fact can be considered as a direct proof of these waves.

Let us first consider waves of a very small amplitude. It is well known that no atmosphere is transparent to a progressive wave, unless its period is smaller than a critical period.

$$P_{\text{crit}} = \frac{4\pi H}{a},$$

where  $H$  is the scale height of the atmosphere and  $a$  the sound velocity. For an isothermal atmosphere

$$H = \frac{a^2}{\gamma g}, \quad \text{so} \quad P_{\text{crit}} = \frac{4\pi a}{\gamma g}.$$

For the sun

$$P_{\text{crit}} \simeq 240 \text{ s.}$$

If at some place in an atmosphere the density is  $\varrho_0$  and the velocity of the material is  $v$ , the flux of mechanical energy  $F_M$  is

$$F_M = \frac{1}{2} \varrho \cdot v^2 V_{\text{group}} = \frac{1}{2} \varrho_0 v^2 a \sqrt{1 - \left( \frac{\sigma_{\text{crit}}}{\sigma} \right)^2},$$

where  $\sigma = 2\pi/P$ .

It is clear that mechanical energy can be carried in the chromosphere only by waves of a period  $P < P_{\text{crit}}$ , the group velocity vanishing for  $P = P_{\text{crit}}$ . A rough evaluation, based on a schematic theory of turbulence in the convective

zone, shows that the main contribution to the mechanical flux is due to periods appreciably smaller than 240 s, of the order of 50 s or smaller.

SCHATZMAN (1949*b*) has even suggested that the efficient acoustic waves have a period of only 8 s. The wavelength corresponding to a period  $P$  is

$$\lambda = 4\pi H \left( \left( \frac{P_{\text{crit}}}{P} \right)^2 - 1 \right)^{-\frac{1}{2}}.$$

It is readily seen that a reasonable approximation, for  $P \ll P_{\text{crit}}$ , is

$$\lambda \simeq aP.$$

If  $P$  is 10 s,  $a = 6$  km/s,  $\lambda = 60$  km  $\simeq \frac{1}{2}H$ .

We shall discuss later the question of the period of the acoustic waves. Let us consider first the increase of amplitude of the wave as it propagates in the atmosphere.

The amplitude increases as  $\exp[\frac{1}{2}(x/H)]$ . From the top of the convective zone to a height of 1000 km, we have about 10 scale heights, and the amplitude should be multiplied by 150, if the phenomenon was still linear.

However, we change from a linear phenomenon to a non-linear one, when the quadratic terms are of the order of magnitude of the linear terms in the equation. Let us consider, for example, the continuity equation

$$\varrho(1 + \text{div } \xi) = \varrho_0,$$

and develop it to the second order

$$\varrho = \varrho_0[1 - \text{div } \xi + (\text{div } \xi)^2].$$

The condition for the transformation of the wave into a shock-wave is

$$|\text{div } \xi| \simeq 1.$$

As we have

$$\xi = \xi_0 \exp \left[ \frac{1}{2} \frac{x}{H} \right] \cos \left( \sigma t - x \sqrt{\frac{\sigma^2}{a^2} - \frac{\gamma^2 g^2}{4a^4}} \right),$$

we find for  $|\text{div } \xi|$

$$|\text{div } \xi| = \xi_0 \exp \left[ \frac{1}{2} \frac{x}{H} \right] \sqrt{\frac{1}{4H^2} + \frac{\sigma^2}{a^2} - \frac{1}{4H^2}} - \frac{\sigma}{a} \xi_0 \exp \left[ \frac{1}{2} \frac{x}{H} \right].$$

The condition  $|\operatorname{div} \xi| = 1$  gives, with  $\sigma \xi_0 = \frac{a}{v}$ ,

$$X = 2H \ln \frac{a}{v},$$

with  $v = 1 \text{ km/s}$ , we have

$$\frac{X}{2H} = 1.8.$$

We can conclude that in less than 4 scale heights, the wave becomes a shock-wave.

If there were no dissipation, the amplitude of the wave would then be very large, the material velocity being of the order of the sound velocity or larger. However, as there is energy dissipation, the velocity amplitude of the wave does not exceed the sound velocity, and it remains small.

This is the main discrepancy between Schatzman and Biermann's theories. As UNSÖLD recalls it (1960), BIERMANN supposes that dissipation occurs when the Mach number is of the order 1. However, the shock front appears certainly before such a large amplitude is reached, as the velocity of propagation is larger in the regions of compression than in the regions of dilatation. As an exact theory does not exist, we satisfy ourselves by a comparison with the uniform case, where the shock front appears after a distance  $x'$ :

$$x' \simeq \frac{a}{v} \frac{aP}{2\pi},$$

with  $aP/H \simeq \frac{1}{2}$ ;  $a/v \simeq 6$ , we obtain  $x'/H = \frac{1}{2}$ .

Therefore, we shall consider that already in the photosphere, the compression waves are transformed into shock-waves.

After two scale heights, the velocity in the wave is about  $(a/c)$ , (Mach number  $M = 1/c$ ), but dissipation in the front is already present.

Dissipation occurs in the shock front, as a consequence of the steep change in density. It is worth considering the theory of dissipation for a viscous fluid.

The energy dissipated per second is

$$\int \frac{4}{3} \mu \left( \frac{\partial u}{\partial x} \right)^2 dx = \frac{1}{\gamma - 1} \int \left( u \frac{dp}{dx} - \frac{\gamma p}{\rho} u \frac{d\rho}{dx} \right) dx,$$

where  $\mu$  is the coefficient of viscosity. The change of specific entropy being

$$dS = c_p \left( \frac{dp}{P} - \gamma \frac{d\rho}{\rho} \right),$$



we see that the energy dissipated in the shock front is given by

$$\int \rho T u \, dS,$$

$T \, dS$  is the change of energy per gram.  $\rho T \, dS$  is the change per cubic centimeter, and  $u \rho T \, dS$  the change per square centimeter per unit of time.

In the case of infinitely weak shocks, the energy dissipated is  $\rho_0 T_0 u_0 \Delta S$ . It is well known that  $\Delta S$  is then proportional to  $(\Delta p)^3$ . We are led to a formula which is similar to the formula given by BRINKLEY and KIRKWOOD (1948), and to the formula used by SCHATZMAN (1949), by DUBOV (1960) and WEYMANN (1960)

$$(1) \quad \Delta W = -\frac{\gamma + 1}{12} \rho_0 \frac{(\Delta V)^3}{V},$$

where  $\Delta V$  is the velocity behind the shock front and  $V$  the sound velocity. The matter is supposed to be at rest ahead of the shock front.  $\Delta W$  is the energy dissipated for 1 cm of propagation.

The question now is naturally of finding the energy  $W$  corresponding to the dissipation  $\Delta W$ . If we can suppose that we have N-shaped waves, we have simply

$$W = \frac{1}{2} \rho_0 (\Delta V)^2 V t_0,$$

where  $V t_0$  is the length between two successive shock fronts (DUBOV, 1960). Using a similarity argument, SCHATZMAN (1949) was led to a similar formula, but his time  $t_0$  was not rigorously a constant.

The choice of  $t_0$  is naturally very important, as it relates the flux of mechanical energy and the rate of dissipation. DUBOV (1960) takes  $t_0 = 10$  s; SCHATZMAN (1949) as mentioned above, takes  $t_0 = 8$  s.

UNNO and KAWABATA (1955) deduced from the theory of turbulence in the convective zone  $t_0 = 4.6$  s.

As mentioned by DE JAGER (1961)  $V t_0$  is likely to be the length of the wake behind the shock front.

In the case of N-waves, the velocity behind the shock front is related to the mean square velocity  $\overline{W^2}$  by the relation

$$(\Delta V)^2 = 9 \overline{W^2}.$$

If  $E$  is the energy radiated away per gram per second, we have the relation

$$W = \left( \frac{E V t_0}{3} \right)^{\frac{1}{3}}.$$

For  $E = 10^{10}$  erg g<sup>-1</sup> s<sup>-1</sup>,  $V = 6 \cdot 10^5$  cm s<sup>-1</sup>, we find  $W = 2.2$  km/s, corre-

sponding to  $\Delta V \simeq V$ . For such a velocity, the shock cannot be considered any more as a weak shock. However, the approximate formula (1), is still a good approximation, as has been shown by SCHATZMAN (1949 *b*).

In fact, an exact value of  $W$  can be found only as a result of the theory of transfer in the low chromosphere. The equilibrium theory of the chromosphere supposes an exact balance between the heat generated by shocks and the energy radiated away.

An interesting remark has been made by DUBOV (1960), supposing that the energy is radiated away either by hydrogen or by helium. He shows that if the energy dissipated by acoustic waves increases, the temperature has to jump from about 6000 to 12000. He suggests that the appearance of the spicules is due to a rapid change in the thermal balance from a «cold» to a «hot» plasma. However, his results should be revised, in order to take into account the exact solution of the non-local-thermodynamic-equilibrium conditions, as for example in POTTASCH and THOMAS (1960).

Compression waves can dissipate energy, as long as the mean free path is not too large. In the corona, where the conductivity of the gas becomes very large, there is no dissipation any more by shock-waves, and the corona becomes almost isothermal. Already mentioned by ALFVÉN (1941), the effect of conductivity has been especially taken into account by SCHATZMAN (1949) and recently studied in more detail by UNSÖLD (1960).

Dissipation in magnetohydrodynamic waves has been considered by PID-DINGTON (1955 *a* and *b*, 1956) and by COWLING (1956). The main effect, in transverse waves, is due to the fact that all particles (neutral atoms, ions and electrons) do not move exactly together. The calculation of the coefficient of damping of transverse waves by neutral friction has been done by Miss A. BAGLIN (1960), starting from the microscopic theory of a plasma with a high number of collisions.

If  $\nu_{\alpha\beta}$  is the number of collisions per second of one particle of species  $\alpha$  against all particles  $\beta$ , we have for the constant of damping:

$$-K_{mag} = \omega^2 \left\{ \frac{n_a}{n_i} \frac{\omega_p}{c\nu_{ae}(1 + n_a/n_i)^{\frac{1}{2}}(\omega_L\Omega_L)^{\frac{1}{2}}} + \right. \\ \left. + \frac{(\nu_{ei} + \nu_{ie} + (\nu_{ia}\nu_{ae} + \nu_{ea}\nu_{ai})/(\nu_{ai} + \nu_{ae}))\omega_p(1 + n_a/n_i)^{\frac{1}{2}}}{c(\omega_L\Omega_L)^{\frac{1}{2}}} \right\},$$

where  $\omega_p$  is the plasma frequency

$$\omega_p^2 = \frac{4\pi n_e e^2}{m_e},$$

$\omega_L$  and  $\Omega_L$  the gyrofrequency of the electrons and the ions.

Numerically, we find

$$-K_{\text{mag}} = \omega^2 \left\{ 10^{-1.93} \frac{1-x}{x} \frac{1}{BT^{\frac{1}{2}}N^{\frac{1}{2}}} + 10^{-30.35} \frac{1-x}{x} \frac{N^{\frac{3}{2}}T^{\frac{1}{2}}}{B^3} + 10^{21.17} \frac{N^{\frac{3}{2}}T^{-\frac{3}{2}}}{B^3} \right\}.$$

$x$  being the degree of ionization.

If we compare  $K_{\text{mag}}$  to the path of a transverse wave in a time  $t_0$ , we obtain

$$\frac{1}{\omega^2} |K_{\text{mag}} V_A t_0| = 10^{10.51} \frac{1-x}{x} \frac{1}{NT^{\frac{1}{2}}} + 10^{-18.01} \frac{1-x}{x} \frac{NT^{\frac{1}{2}}}{B^2} + 10^{-8.83} \frac{NT^{-\frac{3}{2}}}{B^2}.$$

The velocity of the transverse wave is equal to the velocity of the Alfvén wave,  $B/\sqrt{4\pi\rho}$ , where  $\rho$  is the total density of the gas.

The consequences of the above expression have not been worked out yet. However, it can be seen that no transverse wave can propagate in the lower chromosphere, unless the magnetic field is large enough to prevent complete damping. For example, for  $N = 10^{16}$ ,  $(1-x)/x = 10^{3.5}$ ,  $T = 6000^\circ$ ,  $\omega = 0.6$ , the second term gives

$$|K_{\text{mag}} V_A t_0| \simeq \frac{800}{B^2}.$$

Roughly speaking, the magnetic field has to be larger than 30 G for magnetohydrodynamic waves to propagate in the photosphere. The appearance of MHD waves in the upper chromosphere can explain the transfer of mechanical energy in the corona.

In the regions of low density, the damping of MHD waves becomes very small, unless we have to deal with shock waves. The production of a shock results from the fact that the plasma is compressible.

Let us consider, with K. O. FRIEDRICHS (1959) a surface  $S(t)$  with a characteristic velocity of propagation,  $c_{\text{ch}}$ , in the normal direction at each point of the surface  $S(t)$ .

If we consider the normal component of the flow velocity

$$u_n = (\mathbf{n}_0 \cdot \mathbf{p}),$$

we can write for the characteristic velocity

$$c_{\text{ch}} = u_n \pm c.$$

Thus,  $\pm c$  is the normal component of the characteristic velocity, relative to the flow velocity.

As is well known, there are, at any point, three values of  $c$ . The flow

velocity  $u$  can be considered as the composition of three velocities; one,  $u_t$ , is a transverse velocity; the two others being along the two vectors  $\alpha$  and  $\beta$ :

$$\left\{ \begin{array}{l} \alpha = \frac{H_n \mathbf{H}}{4\pi} - \rho c_{\text{fast}}^2 \mathbf{n} \\ \beta = \frac{H_n \mathbf{H}}{4\pi} - \rho c_{\text{slow}}^2 \mathbf{n} . \end{array} \right.$$

where  $c_{\text{fast}}$  and  $c_{\text{slow}}$  are the two velocities of propagation of the non-transverse waves. The third velocity is

$$b_n = \left( \frac{H_n^2}{4\pi\rho} \right)^{\frac{1}{2}},$$

and is the Alfvén velocity.

Except for the pure transverse wave, the characteristic velocity differs from the velocity  $c$ . Therefore, exactly as for sound waves, these waves will have the tendency to get steeper and steeper, until they become shock-waves.

The transverse wave, on the other hand, being a shear wave, is not associated with a change of density, and has no reason for becoming a shock-wave. Moreover, in case of a transverse shock-wave, there is no change of density and no change of entropy, and therefore, no dissipation in the shock front (except when taking into account the diffusion of each kind of particles with respect to the others). Therefore, only MHD compression waves can lead to a shock and to large dissipation.

OSTERBROCK (1961) has studied in detail the dissipation by MHD shocks.

**1'4. Stellar chromospheres and corona.** — It seems very likely that for stars of late spectral types, which have a convective zone, a source of energy exists which can produce around these stars a chromosphere and a corona.

Several problems arise in that connection, which can be mentioned only briefly:

(i) In giant stars, it seems probable that a large amplitude of the acoustic waves (shock-waves), is reached already in the photosphere. Assuming that it is the case, SCHATZMAN (1949 *a*) has shown that a flux of mechanical energy  $F_m$  of the order of 1/25th of the total energy flux can provide sufficient energy for the production of large chaotic motion. It is then possible to explain the width of the lines in several stars ( $\delta$  *C Ma*,  $\epsilon$  *Aur*,  $\eta$  *Aql*,  $\alpha$  *C Mi*).

(ii) However, it is quite likely that the emission features in the lines of Ca II, found by O. C. WILSON and M. K. VAINU BAPPU (1957) are a consequence of the temperature gradient in the outer layers of the star, and are similar to the emission feature in the case of these lines on the sun. JEFFERIES



and THOMAS (1959) have shown, in the case of the sun, that these features can reasonably be explained by the temperature gradient.

The existence of such a gradient shows that most stars are surrounded by a chromosphere.

(iii) The study of the transfer problem with an energy source leads to new solutions with a temperature minimum in the outer layers of the star. BAROIN and SCHATZMAN (1950) have obtained a model with a temperature minimum at

$$\bar{\tau} \simeq 0.02.$$

New computations of such models, with solution of the problem of line formation, should be made.

Work in that direction lies in the recent paper of WEYMANN (1960). However, he did not consider flows with a discontinuity, as studied by PARKER. Therefore, his conclusions concerning the optical effects of the outgoing flow of matter cannot be considered as definitive.

(iiii) It should be mentioned that mass-loss occurs as soon as the thermal velocities of the particles is of the same order of magnitude as the velocity of escape, as mentioned by RUBRA and COWLING (1960). In supergiants, the temperature corresponding to escape can be reached before the temperature of a corona, as we have

$$T_{\text{escape}} = 10^{7.2} \frac{m}{m_{\odot}} \frac{R_{\odot}}{R}.$$

For a radius of a few times  $10^2 R_{\odot}$ , we may well have a temperature of escape of  $10^5$  °C, which is well below the temperature of the corona.

## 2. - Theory of shocks.

Before giving the analysis of the published work on propagation of shocks in a variable density atmosphere, we shall briefly recall some important references concerning shock waves:

### (A) *Shock fronts*

#### (a) Theory of dissipation in a shock front:

LANDAU and LIFSCHITZ (1953);

#### (b) Propagation of shocks in a uniform gas, with dissipation:

BRINKLEY and KIRKWOOD (1948);

## (c) Magnetohydrodynamic shocks:

F. DE HOFFMANN and E. TELLER (1950),

K. O. FRIEDRICHS (1955),

J. BAZER and W. B. ERICSON (1959),

P. GERMAIN (1959);

(d) Influence of radiation. Work of SACHS (1946) and ROSSELAND (1949) gives the relations between densities, pressures, temperatures, and velocities, before and after passage of the shock-wave. SCHATZMAN (1951) has calculated the velocity of propagation of a shock-wave, taking into account the relativistic effects.

Let us call  $U$  the velocity of the shock front,  $U-u$  the material velocity behind the shock front,  $\varrho_1$  and  $\varrho_0$ ,  $P_1$  and  $P_0$ ,  $T_1$  and  $T_0$  the density, pressure, and temperatures after and before passage of the shock front. If we call  $x$  and  $y$  the ratios of the densities and temperatures

$$T_1 = yT_0, \quad \varrho = x\varrho_0,$$

and

$$1 - \beta = P_R / (P_R + P_0),$$

we have the relation between  $x$  and  $y$

$$x^2 y \beta_0 + x [(7 + y^4)(1 - \beta_0) + 4(1 - y)\beta_0] - (7y^4 + 1)(1 - \beta_0) - \beta_0 = 0,$$

the velocity  $U$  is given by

$$U^2 = \frac{P_0}{\varrho_0} \cdot \frac{\beta_0 xy - 1 + (1 - \beta_0)y^4}{x - 1},$$

and  $U-u$  is given by

$$x(U-u) = U.$$

Numerical study of these relations is under way.

H. K. SEN and A. W. GUESS (1957) have studied the problem of radiative transfer in a shock front. Their work is based entirely on the assumption of local thermodynamic equilibrium and great optical thickness. The result is expressed in terms of thickness of the shock front as a function of the particle mean free path,  $\lambda_0$  ahead of the shock:

$$\text{thickness} = t_0 \lambda_0.$$

The following table is taken from the Sen and Guess paper, where  $M_0 = u_0/c_0$  is the Mach number for the velocity  $u_0$  of the matter with respect to the front and ahead of it (Table I)

TABLE I.

$M_0$	$t_0$	$t_{0R}$
1.5	9.5	27.3
2	8.5	31.4
2.5	9.7	40.8
4	14.7	87.8

$t_0$  is the thickness without radiation,  $t_{0R}$ , with radiation.

The exactness of these results can be contested, as MARSHALL (1956) has shown. However, SEN and GUESS, consider the possibility that electrons and ions are not at the same temperature in the front, in which case the Prandtl number

$$P_r = \frac{\mu \gamma c_v}{k},$$

(where  $\mu$  is the coefficient of viscosity) is  $\frac{3}{4}$ . If electrons and ions were at the same temperature,  $P_r$  would be much smaller than  $\frac{3}{4}$ . But, if ions and electrons are not at the same temperature, what is the meaning of using the Rosseland mean calculated for L.T.E.?

KAPLAN and KLIMISHIN (1959) have also calculated some of the properties of shock-waves, including radiation, with special regard to the detonation-recombination wave.

KUBIKOWSKI (1959) has studied the cooling of matter behind the shock front when the optical thickness of the matter ahead of the shock front is small, for the purpose of application to cepheids. He obtains an expression for the distribution of temperature behind the shock front, a characteristic length being

$$l = \frac{1}{\kappa_Q} \left( \frac{c_p \bar{\mu} P_g u}{R 6 P_R c} \right)^{\frac{1}{2}},$$

where  $u$  is the velocity of the shock front with respect to the matter behind.  $\kappa_Q l = \tau_s$  is the optical thickness of the region of decay of the temperature.

For example, for  $\log \varrho_2 = -8.89$ ,  $\theta_0 = 0.16$ ,  $\log P_g = 2.57$ , we have  $\log \kappa_2 = -0.41$  behind the shock front,  $c_p \mu / R = 16$ . With  $u = 5$  km/s, we obtain

$$\tau_s = 0.02.$$

A similar problem has been studied by KAPLAN and KLIMISHIN (1960), with accent on the heating of the gas ahead of the shock front.

Revision of the theory is needed.

(B) *Shocks in variable density atmosphere.*

(a) Method of similarity. Already, at the first meeting, BURGERS (1949) discussed the problem of propagation of a shock-wave in a variable density atmosphere. Since that time, the method of similarity has been developed by SEDOV (1957). It has been used several times, for example by SEDOV (1955), by KOPAL (1954), and CARRUS, FOX, HAAS, KOPAL (1951 *a, b*), and by M. H. ROGERS (1957) for an infinitely strong shock (gravity being negligible).

For spherical shocks, the similarity method can be applied only for a distribution of density and other parameters given by a power law, *e.g.*,  $\rho = Ar^{-\alpha}$  where  $A$  and  $\alpha$  are constants. The solution is obtained as a function of time and radius through a function  $\xi = (t\psi/r)$ . Moreover, there must be only two characteristic constant parameters, dimensionally independent ( $A$  is one of these parameters).

This second assumption is very restrictive as we usually have more than one characteristic parameter (except  $A$ ) with different dimensions; for example, the constant of gravitation  $G$ , the energy of the explosion  $E$ , the temperature in the center of the star, and so on.

Therefore, similarity solutions can be obtained only by neglecting some parameters. KOPAL (1954) claims that his 1954 solution is very close to actual shocks; though KAPLAN in his preliminary report doubts that similarity solutions can represent astrophysical phenomena.

The case  $\alpha = \frac{5}{2}$  is singular and allows one to choose three parameters,  $A$ ,  $G$ , and  $E$ , of which only two have independent dimensions:  $E \sim GA^2$ . The equation of the movement of the shock is  $r \sim (GA)^{\frac{2}{3}} t^{\frac{4}{3}}$  (SEDOV 1957, CARRUS, FOX, HAAS, KOPAL 1951 *b*). All parameters (density, velocity, pressures ...) behind shock front depend only on the dimensionless parameter,

$$\eta = r/(GA)^{\frac{2}{3}} t^{\frac{4}{3}},$$

and therefore are similar.

If we have two characteristic parameters  $A$  and  $G$ , then  $E$  depends on the time, but  $\alpha$  is arbitrary (KOPAL, 1956).

If  $\xi = t^{2/\alpha}/r$ ,  $\xi_1$  represents the position of the shock front:

$$r_1 = t^{2/\alpha}/\xi_1.$$

The Mach number of the shock is given by

$$M^2 = \frac{4(3 - \alpha)(\alpha - 1)}{\alpha^2 \gamma \xi_1 \alpha}.$$



There is, inside  $\xi_1$  a sphere  $\xi_2$  which is a contact discontinuity, corresponding to the presence of vacuum inside (ejection of a shell).

We should notice here the constancy of the Mach number during shock propagation. In real stellar conditions, it is certainly not true.

A series of papers are devoted to the applications of the method of similarity solutions to the movement of shocks in stars. To the above mentioned papers, we must add SEDOV (1956), JAVORSKAYA (1956), LIDOV (1957), ROGERS (1956). The book of BAUM, KAPLAN and STANYKOVICH (1958) collects a number of important results.

Special applications of the theory of similarity flow has been made to the motion of the shock near the surface of a star (GANDELMANN, FRANK-KAMENETZKY, 1956). It was shown that the equation of motion of the shock near the surface is  $(R - r) \sim t^{0.59}$ . The numerical value 0.590 of the power of  $t$  was found for the stellar envelope with the Kramers law of opacity. In that solution, the velocity and temperature behind the shock increases to infinity when the shock approaches the surface. As a result from the above-mentioned work, radiation would change considerably this result.

(b) Discontinuous medium. Another method, and still an exact method, consists in replacing the variable density medium by a series of layers with different densities. The problem is then to study the effect of passage and reflection across the discontinuities, and this method was suggested at the first meeting (1949). It has been used by CHISNELL (1955) and applied by a group of Japanese scientists (ONO, SAKASHITA and YAMAKAZI, 1960) to the propagation of plane shocks in a plane atmosphere. They show that the intensity of the shock is approximately proportional to the power — 0.6 of the pressure ahead of the shock front, and therefore increases considerably when approaching the surface.

This method is more elaborate than the similarity method and can be applied to a larger variety of cases. It could be improved by introducing radiative loss.

An important work has been done by HAZLEHURST (1961) in order to explain the novae ejection.

(c) Weak shocks. Motions of weak shocks, as shown by WHITHAM (1953), can be investigated with the linearized equations.

SCHATZMAN (1954) has used a Fourier analysis to study the propagation of a given perturbation in an atmosphere. It is worth giving the result, as it has some implications for the heating of the solar chromosphere. The amplitude of the wave can be written

$$S = S(\omega) \exp \left[ \frac{\gamma g z}{2a^2} + i \left( \sigma t - i \sqrt{\frac{\sigma}{a^2} - \frac{\gamma g}{4a^4} z} \right) \right].$$

If we suppose a displacement at  $z=0$ :

$S=0$  for  $t<0$ ,  $S=1$  for  $0<t<\theta$ ,  $S=0$  for  $t>\theta$ , we have

$$S(\omega) = \frac{\exp[i\sigma a] - 1}{4\pi i \sigma},$$

and by integration over  $\sigma$ , we find the amplitude

$$S = \exp\left[\frac{\gamma g z}{a^2}\right] - 1,$$

for  $at < z < a(t+\theta)$ .

The increase of pressure is

$$\Delta P = Cte \gamma a^2 \varrho_0 \left(1 - \exp\left[-\frac{\gamma g(z-z_0)}{a^2}\right]\right).$$

As a function of time, the relative decrease of the pressure behind the shock front is characterized by a time

$$\delta = \frac{a}{\gamma g}.$$

For the sun,  $\delta \simeq 20$  s. This characteristic value is an essential result of the structure of the atmosphere, and is much smaller than the critical period of the atmosphere (indeed,  $4\pi$  times smaller). It corresponds very closely to the period which had to be introduced in the decay theory in order to express in a simple way the kinetic energy of the shock.

In his preliminary report, Kaplan mentions, in connection with the problem of weak shocks, a work of PICKELNER (1959) in which he studied the gravitational damping of acoustic waves.

(d) Solitary waves. The theory of simple waves (Riemann solution) is well-known. BAUM, KAPLAN, and STANYKOVICH (1958) have studied the movement of these waves in a gravitational field. They can show how long it takes for a non-linear flow to turn into a shock. If the initial pressure is  $P_i$ , the velocity of sound  $C_i$ , and the gravity  $g$ , the disturbance of the pressure turns to a shock at the point where the pressure is  $P_s$ .

If  $\tau$  is a characteristic time of the disturbance of the pressure, we have for  $\gamma = \frac{5}{3}$

$$\frac{gP_s}{4P_i} - 1 = \frac{C_i}{5g\tau} \left[1 - \left(\frac{P_s}{P_i}\right)^{\frac{1}{5}}\right].$$

(e) Approximate methods. LEBEDINSKY (1946) and SCHATZMAN (1951) have applied the law of conservation of energy, with the result that the velocity of propagation is given by  $v \sim (\varrho r^2)^{-\frac{1}{2}}$ .

However, the assumption of conservation of energy is questionable in case of strong shocks.

ODGERS and KUSHWAHA (1957) assumed the constancy of the pressure-time curve for every element of the gas. They found a fast damping of the isothermal shocks.

SAKURAI (1956) found a solution for the equation of the movement of the shock created (with energy  $E$ ) in the center of a polytropic sphere. The solution is given by a series in  $(r/r_0)$ , where  $r_0 = (E/3\pi p_c)^{1/3}$ ,  $p_c$  being the pressure at the center of the star. The series converges for  $(r/r_0) < 1$ , and therefore is not applicable to the movement of the shock in the outer layers of a star.

WEYMANN (1960 *b*) takes an average of the equation of energy for N-waves, assuming a profile for these waves. The result is an equation of energy which allows one to calculate the heat transfer in the chromosphere:

$$\frac{d}{d\xi} \left[ \frac{\sigma^3}{12} \left( \frac{F_0^3 \tau_0}{\gamma} \right)^{\frac{1}{2}} \right] + \frac{F_0}{\tau_0} = 0,$$

where  $\xi$  is the Lagrangian co-ordinate,  $F_0$  the average radiation loss,  $\tau_0$  a reference specific volume, and  $\sigma = (p_2 - p_1)/p_0$  is the shock strength parameter.

However, he has not taken into account the refraction of the waves, which was considered by SCHATZMAN (1949 *b*). Due account of the refraction can be found both in DE JAGER (1961) and OSTERBROCK (1961) papers.

### 3. - Conclusion.

Much progress has still to be done in the theory of propagation of shock waves in variable density atmospheres. Much attention should be given to the numerical work of WHITNEY, using the theory of characteristics.

The astrophysicists wish certainly to receive some help from the aerodynamicists to succeed in solving one of the major problems of astrophysics.

### BIBLIOGRAPHY

- ALFVÉN H., 1941, *Ark. Mat. Astr. Och. Fys.*, **27**, A no. 25.  
 BAGLIN A., 1960, *Jour. de Phys.* (in proofs).  
 BAROIN M. and SCHATZMAN E., 1950, *C. R.*, **231**, 757.  
 BAUM PH. A., KAPLAN S. A. and STANYKOVICH K. P., 1958, *Introduction to Cosmical Gas Dynamics*, Moscow.  
 BAZER J. and ERICSON W. B., 1959, *Ap. J.*, **129**, 758.

- BIERMANN L., 1939, *Z. f. Ap.*, **18**, 344.  
 BIERMANN L., 1948, *Z. f. Ap.*, **25**, 161.  
 BRINKLEY S. R. and KIRKWOOD J. G., 1947, *Phys. Rev.*, **71**, 606.  
 BURBIDGE G. R., BURBIDGE E. M., HOYLE F. and FOWLER W. A., 1957, *Rev. Mod. Phys.*, **29**, 547.  
 CARRUS P. A., FOX P. A., HAAS F. and KOPAL Z., 1951a, *Ap. J.*, **113**, 193.  
 CARRUS P. A., FOX P. A., HAAS F. and KOPAL Z., 1951b, *Ap. J.*, **113**, 496.  
 CHISNELL R. E., 1955, *Proc. Roy. Soc. A.*, **232**, 350.  
 COLGATE S. E., 1959, *Phys. Fluids*, **2**, 485.  
 COWLING T. G., 1956, *M. N.*, **116**, 114.  
 DE HOFFMANN F. and TELLER E., 1950, *Phys. Rev.*, **80**, 692.  
 DE JAGER C., 1961, preprint.  
 DUBOV E. E., 1960, *News of Crimean Astrophysical Observatory*, **22**, 101.  
 FRIEDRICH K. O., 1959, *Non-linear wave motion in magnetohydrodynamics*, Center Report No. 1845, Los Alamos.  
 GANDELMANN G. M. and FRANK-KAMENETZKY D. A., 1956, *C.R.U.R.S.S.*, **107**, 811.  
 GERMAIN P., 1959, *N.E.R.A.*, Publ. No. 97.  
 HAZLEHURST J., 1961, *Progress in Astronomy and Astrophysics* (in the press).  
 JAVORSKAYA I. M., 1956, *C.R.U.R.S.S.*, **111**, 783.  
 JEFFERIES J. T. and THOMAS R. N., 1959, *Ap. J.*, **129**, 401.  
 KAPLAN S. A. and KLIMISHIN I. A., 1959, *A.J.U.R.S.S.*, **36**, 410.  
 KAPLAN S. A. and KLIMISHIN I. A., 1960, *A.J.U.R.S.S.*, **37**, 281.  
 KOPAL Z., 1954, *Ap. J.*, **120**, 159.  
 KUBIKOWSKI J., 1959, *Ann. d'Ap.*, **22**, 741.  
 LANDAU L. D. and LIFSHITZ E. M., 1953, *Mechanics of Continuous Media*.  
 LEBEDINSKY A. I., 1946, *A.J.U.R.S.S.*, **23**, 15.  
 LIDOV M. L., 1957, *A.J.U.R.S.S.*, **34**, 603.  
 LIEPMANN H., 1952, *ZAMP*, **3**, 321, 407.  
 MARSHALL W., 1956, *Phys. Rev.*, **103**, 1900.  
 MILNE E. A., 1930, *M. N.*, **91**, 4.  
 ODGERS G. J. and KUSHWAHA R. S., 1957, *Ap. J.*, **62**, 95.  
 ONO Y., SAKASHITA S. and YAMAZAKI H., 1960, *Propagation of shock waves in inhomogeneous gases*, preprint referred to by S. A. KAPLAN in his preliminary report.  
 OSTERBROCK O., 1961, preprint.  
 PAYNE-GAPOSHKIN C., 1957, *The Galactic Novae*, Amsterdam - New York  
 PIDDINGTON J. H., 1955a, *M. N.*, **114**, 638.  
 PIDDINGTON J. H., 1955b, *M. N.*, **114**, 651.  
 PIDDINGTON J. H., 1956, *M. N.*, **116**, 314.  
 POTTASCH S. R. and THOMAS R. N., 1960, *Ap. J.*, **132**, 195.  
 ROSSELAND S., 1946, *Ap. J.*, **104**, 329.  
 ROSSELAND S., 1949, *Pulsation Theory of Variable Stars*, London.  
 ROGERS M. H., 1956, *Proc. Roy. Soc. A.*, **235**, 120.  
 ROGERS M. H., 1957, *Ap. J.*, **125**, 478.  
 RUBRA F. T. and COWLING T. G., 1960, *Mem. Soc. Roy. Sci. Liege*, **16**, 274.  
 SACHS R., 1946, *Phys. Rev.*, **69**, 514.  
 SAKURAI A., 1956, *J. Fluid Mechanics*, **1**, 436.  
 SCHATZMAN E., 1946a, *C. R.*, **222**, 722.  
 SCHATZMAN E., 1946b, *Ann. d'Ap.*, **9**, 199.  
 SCHATZMAN E., 1949a, *C. R.*, **228**, 814.  
 SCHATZMAN E., 1949b, *Ann. d'Ap.*, **12**, 203.



- SCHATZMAN E., 1951, *Ann. d'Ap.*, **14**, 294.  
SCHATZMAN E., 1954, *Bull. Acad. Roy. Belgique*, **40**, 139.  
SCHATZMAN E., 1958, *Ann. d'Ap.*, **21**, 1.  
SCHWARZSCHILD M., 1948, *Ap. J.*, **107**, 1.  
SEDOV L., 1955, *4-th Meeting on Questions of Cosmogony*, 133.  
SEDOV L., 1956, *C.R.U.R.S.S.*, **111**, 770.  
SEDOV L., 1957, *Similarity and Dimensional Methods in Mechanics*.  
SEN H. K. and GUESS A. W., 1957, *Phys. Rev.*, **108**, 560.  
THOMAS R. N., 1948, *Ap. J.*, **108**, 130.  
UNNO W. and KAWABATA K., 1955, *Publ. Ast. Soc. Japan*, **7**, 21.  
UNSÖLD A., 1960, *Z. f. Ap.*, **50**, 57.  
VORONTZOV-VELYAMINOV, 1948, *Gaseous Nebulae and Novae*.  
WEYMANN R., 1960a, *Ap. J.*, **132**, 370.  
WEYMANN R., 1960b, *Ap. J.*, **132**, 452.  
WILSON O. C. and VAINU BAPPU M. K., 1957, *Ap. J.*, **125**, 661.  
WHITHAN G. B., 1953, *Com. pure and applied math.*, **6**, 397.

## PART III.

### Spherically-Symmetric Motions in Stellar Atmospheres.

#### B. - The Propagation of a Shock-wave in an Atmosphere of Varying Density.

#### Discussion.

*Chairman:* M. KROOK

— L. BIERMANN:

A comment on several points made by SCHATZMAN. First, as regards the energy balance of the chromosphere and corona, it is essential to take account of the energy necessary to maintain the corpuscular radiation as well as the optical radiation. The energy in this corpuscular radiation is of the order  $10^5$  erg  $\text{cm}^{-2}$ , which is comparable with that from the corona by optical radiation. Second is a point discussed by LÜST and myself several years ago: *viz.*, all stars having hydrogen convection zones, where occur velocities of the order a few km/s, must be expected to possess chromospheres and coronas. But for supergiants with large radii, the velocity of escape is much less than for the sun, and possibly these stars have only chromospheres, not coronas. It is known from the general properties of the mechanism of radiation loss that there is a sharp transition between the chromosphere, with  $T$  of the order  $10^4$ , and the corona, with  $T$  of the order  $10^6$ . Thus, a supergiant with escape velocity 20 km/s or so cannot retain a corona with thermal velocity 200 km/s or so. Third, LÜST and myself have reached the conclusion that until one reaches the chromosphere-corona interface, one cannot — except within sunspots — expect to have Alfvén waves; in the low levels of the chromosphere one has practically exclusively sound waves. Fourth, on the relative importance of ambipolar diffusion — again studied by LÜST and myself and by LEHNERT — we have both agreed that Piddington's conclusion is not really sound, that outside the spots the contribution to ambipolar diffusion is not really essential. Fifth, in discussing the evolution of the noise from granulation into shock-waves, the possible influence of a chromospheric magnetic field must be taken into account. Sixth, it is not clear to me how a shock with the small relative amplitude suggested by SCHATZMAN could produce enough dissipation to maintain the energy balance; it seems to me necessary to have material velocities near sonic.

— R. LÜST:

First, two comments on the acoustic noise generation, which in the astronomical literature — following the work by Lighthill and by Proudman (*Proc. Roy. Soc. Lond.*, 1952, A **211**, 564; A **214**, 119) — is usually taken proportional to  $M^5$ . Kulsrud (*Ap. J.*, 1955, **121**, 461) has investigated the effect of the presence of magnetic fields, and finds that magnetic turbulence increases considerably the generation of sound. Provided the magnetic pressure is less than the gas pressure, no Alfvén waves will be generated. On the other hand, note that the  $M^5$  law rests on a discussion of isotropic turbulence. But in the top layers of the hydrogen convection zone, we probably do not have isotropic turbulence — the turbulence element will expand and move outward. We might expect the dependence on  $M$  to become somewhat less. This is a question for the aerodynamicists, the question of the generation of acoustic noise in the presence of a density gradient. Second, consider the question of wave propagation in a magnetic field. We have investigated — results are as yet incomplete — an atmosphere with an outward density gradient and a vertical, constant magnetic field. In the lower layers, we took gas pressure large compared to a magnetic pressure; and in the top layers, the reverse. We assumed cylindrical symmetry, and investigated how an initial pressure impulse propagates outward. We introduced an artificial viscosity into the wave so as to be able to treat shock-waves. One finds indeed that an acoustic wave is guided by a magnetic field, so that the propagation of the wave is preferentially along magnetic lines of force. The difficulties inhibiting the completion of the work are those of boundary conditions at the top of the layer, already discussed at the last session. We made several assumptions to avoid incoming waves; but always in the calculations we found instabilities occurring at the top layers, and these ultimately travel inwards. It is not yet clear to us whether these instabilities are due to assumed incorrect boundary conditions, or if there are really instabilities. So the problem has not been solved, but there appear to be good indications that a magnetic field is able to guide an acoustic wave preferentially along the magnetic line of force. This was our intent, to see if one could interpret solar spicules as arising from such an effect, noting that the spicules are very often tilted in much the same way as coronal rays and that one likes to identify the coronal ray tilt with that of the magnetic lines of force in the solar field.

— E. N. PARKER:

Let me present some ideas on the dissipation of transverse hydromagnetic waves in the solar corona. If we believe in the continual hydrodynamic expansion of the solar corona, then not only is the rate of heating larger,  $10^{28}$  erg/s, than previously estimated considering only radiation and conduction losses,

$10^{27}$  erg/s, but the heating must take place out to distances of several solar radii. The question is whether such extended heating is plausible.

Let us assume that by some means, such as the convection zone and/or the spicules, there are generated at low levels  $10^{28}$  erg/s in hydromagnetic waves, in the general one gauss field of the sun. Below the corona the gas pressure  $p$  is very large compared to the magnetic pressure  $B^2/8\pi$ , of the general solar magnetic field. Thus, for transverse waves with an amplitude  $\Delta B$  comparable to  $B$ , the motion will be essentially incompressible. The transverse incompressible wave will propagate slowly ( $B/\sqrt{4\pi\rho} \ll p/\rho$ ) out along the magnetic lines of force, which presumably extend approximately in the radial direction. Under such conditions the propagation is without dispersion. The angular frequency  $\omega$  of the waves is of the order of  $(10^{-2} \div 10^{-4})$  rad/s, so that the wavelengths are within a factor of ten of  $(10^2 \div 10^3)$  km. Dissipation due to viscosity and resistivity is negligible.

But as the waves propagate higher in the solar atmosphere, the gas pressure — which is decreasing rapidly — becomes comparable to the magnetic pressure. The medium becomes compressible. It was shown sometime ago by perturbation methods (PARKER, 1958, *Phys. Rev.*) and more recently by reduction to a Riemann-type analysis (MONTGOMERY, 1960, *Phys. Rev. Lett.*) that the hitherto transverse wave will develop a longitudinal or compressible aspect and will rapidly steepen its front. The steepening proceeds without limit, so that eventually some sort of dissipation — resistivity, plasma instability, phase mixing, etc. — must occur, with the result that the energy originally contained in the purely transverse wave is fed into thermal motions.

Note then that if the dissipation and heating of the atmosphere should become so rapid that the gas pressure becomes much larger than  $B^2/8\pi$ , then the steepening of the transverse wave, and the energy dissipation, will slow down. Thus, the dissipation mechanism is self-regulating. Given a large flux of transverse hydromagnetic waves propagating up into the solar atmosphere along the radial lines of force of the general solar field, the temperature of the gas will be elevated by the dissipation of the waves to the point that  $p$  is of the same order as  $B^2/8\pi$ , i.e., the speed of sound will become comparable to the Alfvén velocity. This relation,

$$p = o(B^2/8\pi),$$

will be maintained for as far out in the solar atmosphere as the hydromagnetic wave flux can hold up. In terms of the temperature  $T$  and number density  $N(\rho = 2NkT)$  we have

$$T = o\left(\frac{B^2}{16Nk}\right).$$



which is in rough agreement with observations. For instance, one gauss at the photosphere extending radially outward yields 0.6 G at an altitude of  $3 \cdot 10^5$  km, where  $N \simeq 10^8/\text{cm}^3$ . We compute, then  $T = o(1.5 \cdot 10^6 \text{ }^\circ\text{K})$ . We obtain slightly lower temperatures, lower down, and slightly higher temperatures (up to  $3 \cdot 10^6 \text{ }^\circ\text{K}$ ) farther out.

We suggest that the outer corona is heated principally by the dissipation of initially transverse hydromagnetic waves, so that  $p \simeq B^2/8\pi$  determines the coronal temperature of the sun. We call this regulated heating the *Mach one effect*, which we have already suggested to be operative in the interstellar generated of cosmic rays (PARKER, 1958).

— F. KAHN:

Why does the wave steepen?

— E. N. PARKER:

A quick and purely physical picture is that the velocity of propagation is most rapid where the field is strongest. Let me recall to your minds that the condition which prevails in a transverse hydromagnetic wave is that the total pressure is constant. Therefore, the gas pressure and density must be lower where the field is stronger and that part of the wave propagates faster than the part where the field is weaker and the pressure and density higher.

— A. J. DEUTSCH:

In connection with Biermann's interesting suggestion that there may be giant stars having chromospheres but not coronas, I wonder if escape velocities as low as 20 km/s are possible? For a star of one solar mass to have such, its radius must be 900 times that of the sun. Escape velocities of 100 km/s are possible, possibly 50, but lower than 50 is, I think, impossible.

Second, I would like to ask the following questions in connection with some very qualitative ideas about the nature of the flow processes in the late-type giants, details of which I will discuss in tomorrow's session. Here, one deals with stars having radii several hundred times the solar radius. One knows that matter is streaming out of these stars with velocities of the order of 10 km/s (cf. Table I of Deutsch's talk).

The gas, where we observe it, has an excitation temperature indistinguishable from zero, an ionization temperature which is very low; and a kinetic temperature which cannot be very high. We may call this a corona if we wish, it certainly is very different from the solar corona.

We also know that these stars have emission features which suggest to us that there is a chromosphere. Now we find, by studying the dynamics,

that in order for us to understand the flow at all, we must consider either very high velocities of ejection — which we do not observe — or a very high temperature region — which we also do not observe. Let us suppose, however, that there is a high temperature region near the reversing layer. The temperature need not be as high as  $10^6$ , but it probably will have to be over  $10^5$ . This region is presumably unobserved. I observe the lines produced at greater distances where the gas has cooled off and slowed down.

Is it not reasonable to suppose that the physical mechanism operating here is the following? Due ultimately to convective processes well below the photosphere, acoustic disturbances are set up which then propagate outwards through the reversing layer into the chromosphere, dissipating energy as they go and heating the gas — much the same picture we have for the solar chromosphere and corona. When we reach a sufficiently high level in the « corona », we find that the inhomogeneities due to the acoustic disturbances and shocks are pretty well evened out, and we are left with a medium which is nearly homogeneous, having a high temperature, and having some net velocity outwards. Shortly after we reach that point, we come into the regime where we can observe the gas. Is this a physically realistic picture? If so, then in order to understand it in a more quantitative way, it would be appropriate to formulate these specific gas dynamical problems. First, I would like to consider a one-dimensional problem: I will consider a gas which may have a temperature gradient, and which may have a gravitational field going through it in the «  $x$  » direction. Let us suppose that at  $t = 0$ , I give to a certain slab of gas a certain initial velocity which will be prescribed, and then let it go; I simply assert that at  $t = 0$ , I know the velocity and the mass contained in this slab. And I ask for the subsequent motion of the gas, both in front of the slab, after it has been started on its way, and behind the slab. The particular question which I would like to have answered, because it is one that's relevant to the problem that I have been discussing, is this: as a result of the impulsive motion, how many g/cm<sup>2</sup> of matter will flow through a surface which is well removed from the slab? This effectively gives me the rate of mass-loss in the plane problem. Of course, eventually I will want to consider the same problem with a spherical geometry, where I give a thin shell an impulsive disturbance and ask, as a result of that impulsive disturbance, how many grams of matter flow per second through a larger sphere? I would also like to ask what are the properties of the velocity field and of the mass transport? Will they depend upon the ratio of energy to momentum in the initial disturbance? It seems to me that they should. The characteristics of the flow and of the mass-loss might well be different, depending upon whether I start with 1 g/cm<sup>2</sup> moving at Mach 10, or 5 g/cm<sup>2</sup> moving at Mach 2. It is necessary to ask in what ways this will change the temperature, the velocity field, and the rate of mass transport. These are some of the questions

which I wish to put before the hydrodynamicists as probably relevant to the kinds of flows which we have observed.

— F. N. PARKER:

I would like merely to comment that as an alternative to the ideas that Deutsch has expressed — impulsive motions deeper in the star sending ripples of material which go out into infinity — was the picture that I got from Deutsch's observational description. It is possible, using temperatures which are not in disagreement with what one sees in the atmospheres of these giants, to write down a simple spherically symmetrical steady flow out of the giant star. The hydrodynamic flow is similar to the hydrodynamic expansion of the solar corona (cf. Session III, C). The numbers would be quite different, but the solutions are the same analytical character.

— A. J. DEUTSCH:

Apparently I did not make my point clear. I have in mind a model for the flow in the region where one loses sight of the initial disturbances which are responsible for the transport of energy, momentum and mass at the base of what becomes, at some number of stellar radii, essentially a smooth spherically symmetrical flow.

— R. N. THOMAS:

Three points. First, regarding your conjecture that at a sufficiently high level in the outer atmosphere the inhomogeneities associated with the heating mechanism are essentially evened out, note that in the sun, the one star we observe in detail, we have strong inhomogeneities throughout much of the region in which we observe spectral lines. Indeed, in the region where mechanical effects enter, the lower parts appear to be homogeneous from a momentum-input standpoint; from an energetic, one doesn't yet know. But higher, the spicules appear; spike-like columns, moving outward at (10 : 100) km/s, transporting enough mass to replace the corona every few hours. One might say that these spicules are the «initial state» postulated by DEUTSCH, rather than a uniform, spherically-symmetric slab. So, my second point is that several years ago I did just what Deutsch asked — only in terms of a limited (in area) block of gas rather than a spherically-symmetric flow — assuming an initial velocity for a column of gas, and asked its configuration (1950, *Ap. Journ.*, **112**, 343; earlier rough model 1948, *Ap. Journ.*, **108**, 130). I actually asked for a steady state, so that the model was essentially that of a supersonic jet in a gravitational field, with high Mach number. So the solution was Prandtl's old solution modified by a gravitational field. I did

not push the model further, because it seemed to me it neglected the basic physical features that such a problem must include: viz., strong variation in internal energetic degrees of freedom of the gas (I used constant  $\gamma$ ), and coupling with radiation field (which I ignored). But if one wants to ask about the flow field in a chromosphere-corona, it seems to me that a spicule field is an equally-likely starting point to a spherically-symmetric one, based on our present knowledge. However, I would like to pass no judgment on whether this rough picture of mine, or the acoustic-wave sharpened by magnetic field picture of Lüst, is preferable. Third, relative to Biermann's comment on stars without corona but with chromosphere, regions of  $10^6$  and  $10^4$  °K for the temperature, I would emphasize that the radiative stability arguments underlying this picture would lead also to additional regions having intermediate values of temperature. For the sun, we have evidence for a smooth distribution of  $T_e$  up to about  $1 \cdot 10^4$ ; a jump to somewhere  $(2 \div 5) \cdot 10^4$ ; evidence for values of  $T_e$  in the range  $(0.7 \div 1.5) \cdot 10^5$ ; and it is not clear that there may not be another region between this last and a value  $(0.5 \div 1) \cdot 10^6$ . So one doesn't want to look at the outer atmosphere as a rigorously 2-component affair. It just so happens that most of our observations have emphasized the  $10^4$  and  $10^6$  °K regions of the outer solar atmosphere so far. I think the Wolf-Rayet stars are a good example to keep in mind, where other regions are emphasized; the solar rocket observations are doing the same for the sun.

— M. KROOK:

I must admit to some confusion as to the relative importance of sound waves and hydromagnetic waves. I hope someone will clarify the position where are the sound waves; to what extent are they inadequate for the heating, and to what extent are hydromagnetic waves needed for this?

— E. SCHATZMAN:

First, I would draw a conclusion from what has been said by BIERMANN, DEUTSCH, and PARKER relative to the giant stars. For the sun, the temperature rise in the outer atmosphere comes only in regions which are optically very thin in the visual continuum. But in the giant stars, it is likely that the dissipation behaves much differently, and we may have a rise of temperature beginning already at optical depth 0.2 in the continuum. This result has been obtained using a phenomenological theory of the velocity of propagation, dynamic pressure of the waves, etc. But I think we should add to that result the observation that if we have energy enough, part of the energy is used to push away material, as in Parker's picture of the corona. So I think that if in the region of optical depth 0.2 we would add the energy used to heat up the material and that to push it away, we could develop the theory



to obtain the temperature inversion, the model of the star without a corona, and the mass-loss. Second, on the question raised by KROOK, we have some information from the sun. From Leighton's pictures, for example, we know that we may have certain regions on the solar surface where we observe an increased magnetic field, say 50 G. If we compare such pictures with those of calcium faculae, we find the faculae coinciding with the regions of enhanced field. And there is a sharp transition between these facular regions having field, and non-facular regions showing no field. So, I think we could suggest that outside the faculae, we have heating without effect of the magnetic field; and in the faculae, we have heating including the effect of magnetic field, at heights about 1000 km above the solar photosphere. That is, we have heating by compression waves outside the faculae; while in the faculae, we have to include dissipation by transverse waves. Finally, note that it is only at very great heights, some 2 solar radii, that we might expect to have dissipation possible *only* by transverse waves, because here the mean free path is too large for dissipation by longitudinal waves.

— R. LÜST:

I only emphasize that to get this damping for the Alfvén wave, you have to be in a region where the matter is only partly ionized; for without some neutral gas, the conductivity is not sufficient to make significant damping. The only mechanism that can really contribute is the ambipolar diffusion between neutral and ionized component.

— E. N. PARKER:

I think you are overlooking a lot of possibilities, such as plasma instabilities.

— R. LÜST:

Agreed, there are certainly other possibilities, not yet completely understood, which would contribute to the damping. But I refer just to the Pidington mechanism, on which this necessity of a neutral component is a severe condition.

— C. DE JAGER:

I think it is clear that the facular regions are heated in the photosphere, above optical depth 1; but I wonder if we really can be sure they are heated in the chromosphere? They are a bit brighter in  $H_{\alpha}$  and Ca II, but this does not necessarily mean they are heated more than is the surrounding region. Second, the point was made that the hottest stars should not have coronas

because they do not have convection zones. This would follow if the corona in these stars arose from turbulence generated by convection. But according to Miss UNDERHILL, we observe turbulence in these stars. How it is generated, we do not know; but if it exists, I think it should produce a certain mechanical flux, thus could give a corona. Third, agreed that Alfvén waves can only be generated in regions where magnetic pressure exceeds gas pressure — thus probably only in sunspot regions — there is still a factor not yet mentioned. Such waves can be reflected, in a region of decreasing density. If we compute to see what happens to these waves generated in sunspot regions, we find the greater part reflected backwards. What happens then? The only thing I can imagine is that they are transformed into acoustic turbulence, which leads to heating of the lower parts of the spot. I would pose this problem to the aerodynamicists.

-- A. A. BLANK:

There are three speeds of propagation of hydromagnetic waves. Why is the discussion confined to Alfvén waves; have you thought about the possibility that the propagation could be a more general variety? Even if one begins with a compressionless wave, a pure Alfvén wave generated in the incompressible core, as soon as the wave reaches a higher level where compression is possible, the energy will be propagated in all the available modes. This is not a mere possibility; there is a proof by Grad that, in general, the three modes cannot propagate independently through a compressible medium.

— M. KROOK:

I think one also wants to distinguish the case of a large amplitude disturbance where the resolution and propagation of individual small amplitude modes does not have any direct meaning.

— W. B. THOMPSON:

In particular, these things may happen in the corona where the hydrodynamic picture is questionable because of the long mean-free-path. There are a number of processes, not described by hydrodynamics, which might be important, especially non-collisional damping (Landau damping) which might be great enough to beat any steepening edge.

— A. UNSÖLD:

A remark from the observational viewpoint, relative to velocity fields at the chromosphere-corona boundary. We talked at various times about the

spicule structure of the chromosphere; about these spikes extending some 10 000 km high and moving at some 20 km/s.

These spicules are sometimes quite similar to small prominences; and, in fact, long before one spoke about spicules, one talked about small prominences. It happens quite frequently that they assume more complicated structures and move off with higher velocities. From the spicules there is quite a continuous transition into so-called rising eruptive prominences — large masses of gas apparently of cooler temperatures up in the corona which move with very large velocities and sometimes quite suddenly speed up by hundreds of km/s. I remark that in order to remind also the aerodynamicists that there exists in the outer part of the solar atmosphere a mechanism which is able to accelerate considerable masses of gas suddenly and to very high velocities. It is generally assumed that it is connected with magnetic fields.

— M. KROOK:

Would someone sketch a quick picture of the characteristics of convection zone, photosphere, chromosphere, corona, to give a quick picture of mechanisms of heating and types of motion.

— L. BIERMANN:

*Photosphere outside spot.* — We have turbulence of the order 1 km/s. My own position has been that this is much nearer to turbulence in the aerodynamical sense than to some convection of the Benard type. Densities of the order  $10^{17}$  atoms/cm<sup>3</sup>.

*Photosphere near spot.* — The radiative flow is suppressed to a considerable amount — only  $(20 \div 30)\%$  remains. But photometry shows that practically all the radiation that «disappears» in a spot comes up in a ring around the spot. If you take a total area of spot plus a ring, 15% or so of the radius, around it, then you get practically the same energy as in the non-spot region. So you get the picture of energy being diverted to the sides; it's just a question of how the magnetic field acts in diverting the energy flow beneath the spot. Twenty years ago, it appeared that the material in a spot was essentially quiescent; but now it appears that there are motions of several km/s there. The observational indications suggest a different type of motion inside and outside the spot, even though the velocities are the same size.

*Chromosphere.* — There is a transition region of roughly 300 km between photosphere and chromosphere; and the chromosphere extends up to some 10 000 km. We have velocities that are observed, statistically, to increase upwards from a few to about 20 km/s. There we have velocity fields of a certain scale, structure, and energy, which are connected with the super-

position of sound waves developing into shock waves. On the other hand, THOMAS and colleagues tried many years ago to put forward a picture, starting with the observed properties of spicules at higher levels, and assuming higher velocities of 50 km/s or so at lower levels. To us there is a difficulty seeing how one gets these high velocities at lower levels. LÜST has already described our attempts to produce spicules as a consequence of the action of magnetic fields on the acoustic waves.

*Corona.* — The transition from the upper chromosphere to corona, from a few times  $10^4$  °K to a few times  $10^6$ , is fairly steep. The corona is essentially isothermal, beginning at the level of (10 000  $\pm$  20 000) km. Thus, we have a transition from about (20 000  $\pm$  30 000) °K to  $(1 \pm 2) \cdot 10^6$  in less than 10 000 km. In the corona, there is one serious question: up to now, one has no real evidence of mass motions of the order of the sound velocity there, some 200 km/s. These would be difficult to observe; and observations of line-profiles are not incompatible with their presence; but the question is still open. Theoretically, we would expect them, in order to get the necessary dissipation effects. Of course, in connection with work on plasma physics, we are just beginning to learn about a variety of instabilities; and one may indeed have the result that he obtains much more dissipation than expected on the basis of pure aerodynamics.

(*Ed. note:* It should be noted that the  $T_e$  structure of the chromosphere-corona is presently violently controversial. In the regions called coronal by BIERMANN, values between  $(0.5 \pm 3) \cdot 10^6$  °K are variously given. Arguments for  $T_e$  in the  $10^5$  °K range, occurring as low as 4 000 km, between the spicules, have been given. For the coronal discussion, cf. discussion in Section IV, D (SEATON's remarks) of these proceedings and the *Proceedings of the 1960 Meudon Colloquium on Stellar Atmospheres* (*Ann. d'Ap.*, **23**, 807 (1960)). For the chromosphere-corona transition, cf. *Physics of the Solar Chromosphere*, THOMAS and ATHAY, 1961.)



## PART III.

# Spherically-Symmetric Motions in Stellar Atmospheres.

## C. - Non-Catastrophic Mass-Loss from Stars.

### Summary Introduction.

A. DEUTSCH

*Mt. Wilson and Palomar Observatories*

#### 1. - Introduction.

Gas can be observed to flow from a wide variety of stars into the interstellar medium, and to do so in a fascinating variety of ways. In some flows, the temperature exceeds a million degrees; in others, it cannot be distinguished from zero. Flow velocities range from 5000 km/s down to one or two km/s at the limit of detection. The particle densities in some flows are at least  $10^9$  times larger than in others. The rates of mass-loss span the range from zero to probably more than  $10^{26}$  g/s.

The gas dynamics of most of these flows are still but poorly understood. That the flows must occur widely throughout the galaxy, however, is clear not only from the observation, but as well from our understanding of the laws of stellar structure. These insure the impossibility of any hydrostatic equilibrium in a star more massive than about 1.2 solar masses, once it has exhausted its thermonuclear energy resources. And the time scale for this exhaustion is, for all massive stars, much less than the age of the galaxy. With a recent model of the galaxy, SCHMIDT (1959) found that for every two grams now in the stars, nearly one gram has been ejected from past generations of stars, whose relics are now the white dwarfs. Moreover, the cosmic abundances of the various elements appear to substantiate the view that a large fraction of the matter in the galaxy has already been ejected from stars, in the interiors of which it was subjected to processes of nucleosynthesis (BURBIDGE and BURBIDGE, 1958).

In this review we shall be principally concerned with the recently-discovered ejection processes that occur in the late-type giants and supergiants. Unlike the spectacular mass-loss phenomena which occur in the planetary nebulae, the novae, and the supernovae, these flows are relatively unobtrusive. Despite their comparative mildness, they probably represent the process chiefly re-

sponsible for transferring matter from aging stars back to the interstellar medium. Moreover, conditions in these flows appear to be nearly steady, and this should facilitate their theoretical description. Flows of this kind, in which the gas is cold and the velocities of the order of ten km/s, can be spectroscopically detected in most or all of the stars which lie in the hatched region of the Hertzsprung-Russell diagram of Fig. 1.

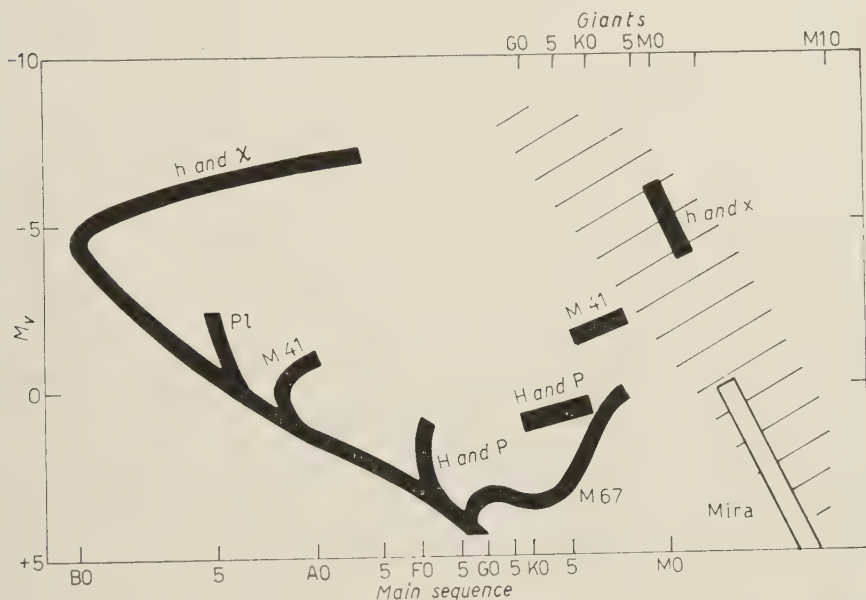


Fig. 1. - Composite HR diagram for open clusters, adapted from SANDAGE (1957). Field stars in the hatched area show circumstellar absorption lines which indicate a slow, cold outflow.

In Section 2, we shall survey the observational evidence for these cold outflows, and in Section 3 we shall review the sparse information available on the thermodynamic variables and their gradients within the flows from the two best-studied stars. In Section 4 we shall then examine the theory of stationary, spherically-symmetric, laminar gas flows under gravity alone, in order to assess what additional theoretical work is needed for an understanding of the observations.

However, before turning to these subjects, we shall look briefly at the other varieties of mass-flows observed in stars. Table I is an attempt to summarize the observations in a form suitable for an orientation to the gas-dynamics of the phenomenon. In each of the flows that are tabulated, the mean-free-paths are likely to be short compared with the scale of the flow, and a hydrodynamical description will therefore be valid for most purposes.

Within any group of objects, like the Be stars, for example, some of the tabulated quantities will differ by factors of three or four from one star to another. These differences override the uncertainties in the values chosen to characterize each class of objects. The second column to the sixth give values

TABLE I. - *Observed properties of outflows from stars ( $R$  is the radial distance at which the outflow is observed).*

Kind of star	$R/R^*$	Flow velocity (km/s)	(1)	Escape velocity (km/s)	Particle density ( $\text{cm}^{-3}$ )	Mass loss ( $M_{\odot}/\text{yr}$ )	References
			Thermal velocity (Km/s)				
Sun	200	500	100	45	100	$6 \cdot 10^{-13}$	PARKER, 1960 <i>a, b</i> CHAMBERLAIN, 1961
		18	30	45	30	$4 \cdot 10^{-15}$	
Close binaries	2	100	25	400	$10^{11}$	$10^{-5}$	SAHADE, 1960 STRUVE, 1958
Be stars	3	50	25	250	$10^{11}$	$10^{-7}$	BOYARCHUK, 1958 SOBOLEV, 1960
WR stars	5	1200	25	500	$10^{11}$	$10^{-5}$	UNDERHILL, 1959, 1960
<i>P Cygni</i> stars	1.3	100	20	800	$10^{11}$	$10^{-5}$	UNDERHILL, 1960, 1961 PAGEL, 1958
M Giants	100	10	4	15	$10^3$	$10^{-9}$	DEUTSCH, 1960
Planetary nebulae	$10^6$	20	20	0.3	$10^4$	$(10^{-1})$	ALLER, 1956 SEATON, 1960
Novae	1 to $10^5$	2000 to 200	20	100 to 5	$10^{10}$ to $10^4$	$(10^{-4})$	GAPOSCHKIN, 1957
Supernovae	?	5000	?	?	?	(1)	GAPOSCHKIN, 1957

(<sup>1</sup>) *Ed. Note:* The temperatures on which these thermal velocities are based have not been determined by one single method, common to all entries. For details on any particular case, inquiry should be addressed to Deutsch.

that refer to the level that is observed in the outflow; typically, the level where the optical depth is of order unity in the spectrum lines produced in the flow. This probably represents a severe idealization; in some cases, it is probable that the physical variables change by several orders of magnitude over the range of distances  $R$  which contribute to the observed spectra.

The last three rows of the table refer to objects in which the mass-loss is catastrophic, or, at least, very far from steady. That is to say, the ejection

process changes appreciably before the ejected matter may be considered to have mixed with the interstellar gas. For these catastrophic processes, the number in the last column gives the total amount of mass loss, in solar units. Considering all the flows together, and recalling that in some Be stars the expansion velocity may vanish or temporarily become negative, we find that the Mach number may clearly be either large, small, or comparable with unity. Similarly, the flow velocity may evidently be either large, small, or comparable with the local velocity of escape.

It is outside the scope of this paper to detail the analytical methods that have led to the results summarized in Table I. The references of column 8 will serve to start the interested reader on a study of the literature bearing on these questions.

## 2. - Circumstellar envelopes of M giants.

In this section we shall briefly review the evidence for cool, quasi-steady outflows of gas from the giants and supergiants in the hatched area of Fig. 1. A more detailed discussion has recently been given by DEUTSCH (1960). The phenomenon was first observed by ADAMS and McCORMACK (1935) in several M-type supergiants. In the spectrum of  $\alpha$  Orionis (M2 Ib), for instance, all strong zero-volt absorption lines were found to be double. Examples may be seen in the enlarged spectrogram of this star in Fig. 2.

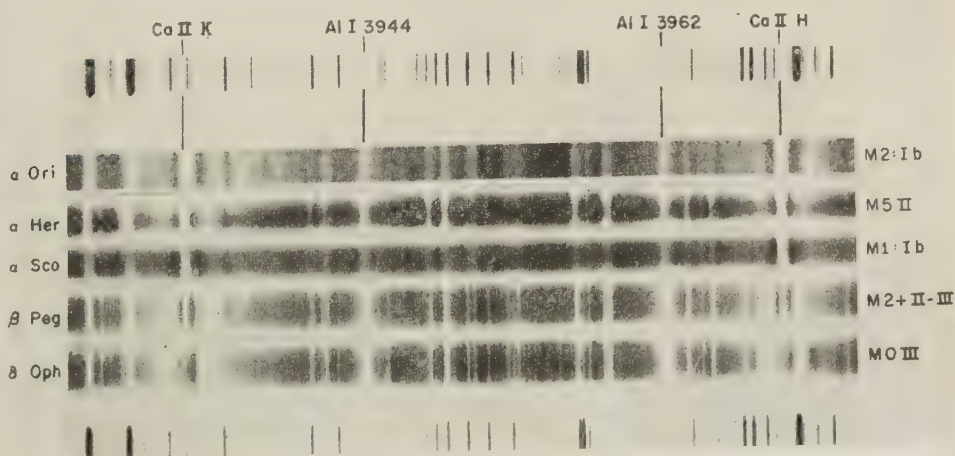


Fig. 2. Enlargements from spectrograms of M stars. Original dispersion, 4.5 Å/mm. The resonance lines of Ca II show circumstellar components in all M giants and supergiants. When these lines are very strong, circumstellar resonance lines of Al I (and other elements) also appear.



One component of these double lines is evidently formed in an expanding shell or envelope, where very low values prevail for the excitation temperature and pressure. This expanding envelope is in a quasi-steady state, for it has given no indication of changes in velocity or other characteristics over the 25 years it has been observed. The other component of the double lines must be attributed to the reversing layer, which also produces all the other relatively wide and shallow lines that characterize the spectrum. These «normal» lines indicate an excitation temperature which lies much closer to the effective temperature of the star. Their radial velocity shows a slow, irregular variation, with a range of about 8 km/s. Relative to the mean velocity of the reversing layer, the circumstellar (CS) lines yield an expansion velocity  $\Delta A$  of 8 km/s.

These spectroscopic observation at once suggest that we have to do with a star which suffers small and irregular pulsations, and which is surrounded by an expanding low-temperature envelope. But the escape velocity at the surface of  $\alpha$  *Orionis* is probably of the order of 100 km/s, and in any case is far greater than the 8 km/s expansion that is observed. It was therefore supposed that the gas must fall back into the star sometime after it is observed in slow ascent. Moreover, since no cases were known where the envelope is in contraction, one also had to suppose that on its return to the star the gas is in an unobservable state of ionization.

However, it was then found by DEUTSCH (1956) that among the CS lines in the spectrum of  $\alpha^1$  *Herculis* (M5 II), with an expansion velocity of 10 km/s, there are many which can also be seen in the spectrum of its visual companion,  $\alpha^2$  *Herculis* (G0 II-III). Since no comparable zero-volt cores are known in other G stars, it was concluded that the circumstellar shell of this M star actually envelopes the visual companion. From the geometry of the visual pair, it could then be inferred that the M star envelope is at least 1000 a.u. in radius, or more than 350 times the radius of the M star. This exceeds by a factor of  $10^4$  the maximum height of a particle in a ballistic trajectory with a radial velocity of 10 km/s at the stellar surface. Moreover, at a distance of 1000 a.u. from the M star, the observed expansion velocity exceeds the local velocity of escape. It was therefore concluded that  $\alpha^1$  *Herculis* loses mass to the interstellar medium in the quasi-steady outflow that produces the CS spectrum.

Subsequent work has isolated several other visual binaries which show the  $\alpha$  *Herculis* phenomenon: the lines produced in the CS envelope of the primary M star, superposed onto the spectrum of the earlier-type companion. Fig. 3(a) illustrates the effect in  $\eta$  *Geminorum*, an M3 II star with a G8 III companion at a projected distance of about 100 a.u. The features produced in the CS envelope are the sharp, deep absorption lines on the shortward (left) edge of the emission lines at Ca II *H* and *K*. Along the line of sight to the M star, these absorption lines indicate an expansion velocity of about 28 km/s.

Only a small number of visual binaries are suitable for observations of this

kind. But there are also a few spectroscopic binaries which can provide evidence on the dimensions of CS envelopes. Fig. 3(b) shows the motion of the CS  $K$ -line relative to the normal reversing-layer lines in the spectrum of one

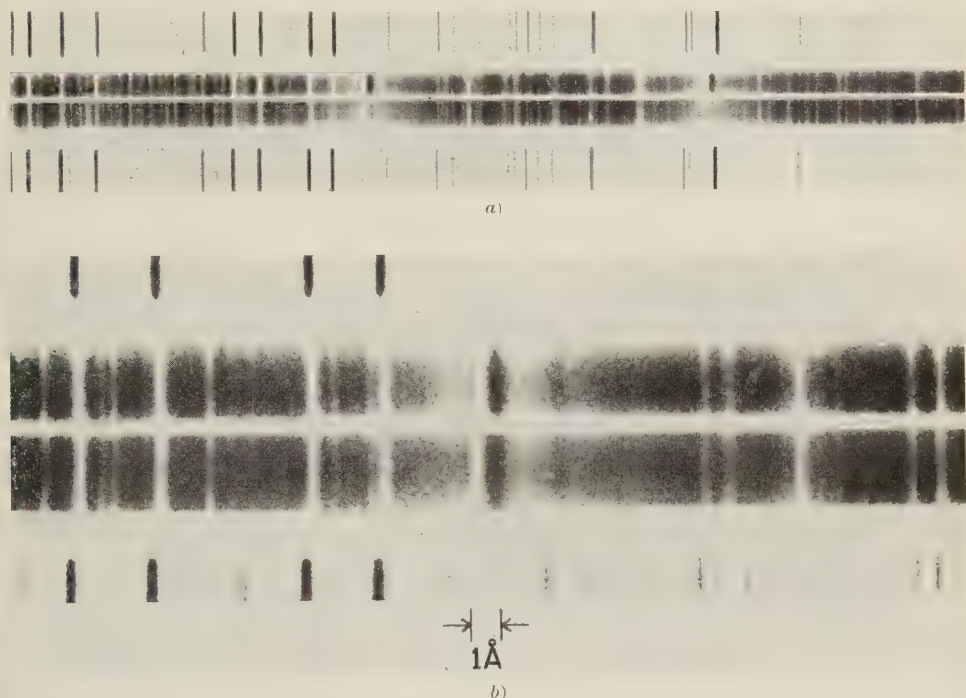


Fig. 3. — *a*) The region of Ca II  $H$  and  $K$  in the spectra of  $\eta$  *Geminorum* (M3 II) and its visual companion (G8 III). *b*) The  $K$ -line in the spectrum of *RR Ursae Minoris* (M5 III); the observations were made at two different phases in the 750 day cycle of this spectroscopic binary. Original dispersion of both plates, 10 Å/mm.

such star. Actually, of course, it is the normal lines which exhibit a variable Doppler shift due to orbital motion; the stationarity of the CS  $K$ -line indicates that it must arise in an envelope which is large compared with the spectroscopic orbit.

Besides these observations of M giants and supergiants in binary systems, high dispersion spectrograms have been obtained for the detection of CS lines in more than a hundred single giant stars of late spectral type. This material indicates that CS lines due to cool expanding envelopes can be recognized in the spectra of all giants later than type M0. Normal (Class III) giants earlier than M0 never show CS features; but comparable lines have been found in a variety of supergiants with earlier types.

The  $H$  and  $K$  lines of Ca II are always the strongest of the CS lines, and often can be seen at a dispersion which is too low to reveal any of the others.

Fig. 2 shows the region of  $H$  and  $K$  in five representative stars. It is important to note that only the sharp, deep absorption components at  $H$  and  $K$  can be attributed to the outflowing CS envelope. These features are normally superposed on emission

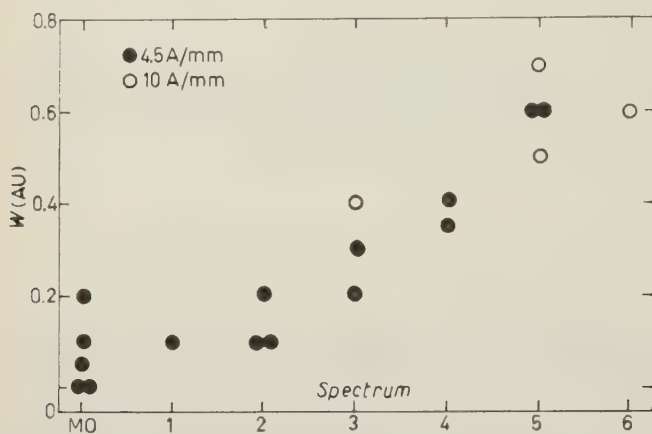


Fig. 4. - The correlation for giants with  $M_v$  fainter than  $-2.5$  of the estimated strength  $W$  of the circumstellar  $K$ -line, and the spectral type.

of luminosity class III, there is a strong correlation between the spectral type and the intensity of the CS  $H$  and  $K$ -lines. This relationship is illustrated in Fig. 4.

The expansion velocities also correlate with spectral type, as is shown in Fig. 5. On the assumption that most of the Ca in the envelope resides in the singly-ionized state, DEUTSCH (1960) has derived the rates of mass-loss which these observations imply. The surface density of Ca II was first obtained from the curve of growth. Since the profiles of the CS lines give no evidence for a velocity gradient in the envelope, the gas density was supposed to vary as  $r^{-2}$ . With the additional assumption

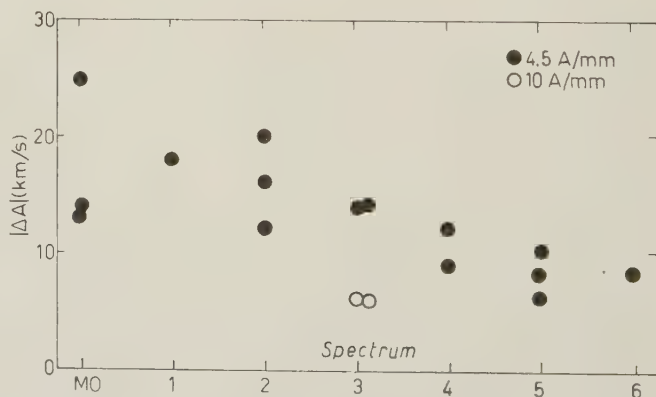


Fig. 5. - The correlation for M-type giants with  $M_v$  fainter than  $-2.5$  of the expansion velocity  $|\Delta A|$  from the circumstellar  $K$ -line, and the spectral type.

the gas density was supposed to vary as  $r^{-2}$ . With the additional assumption

of normal (solar) abundances for other atoms relative to Ca, the rate of mass-loss,  $-\dot{M}$ , could then be derived. At M1, the rate is  $2 \cdot 10^{14}$  g/s; at M3,  $2 \cdot 10^{16}$  g/s; at M5,  $6 \cdot 10^{18}$  g/s. Near type M3, these results are impaired by an extreme sensitivity to the value that is chosen for the Doppler width,  $b$ , of the absorption coefficient. At its worst, this sensitivity, could cause the result for type M3 to be wrong by a factor of ten in either direction.

The correlation in Fig. 4 may indeed indicate that the cooler stars support more massive outflows, in accord with these estimates by DEUTSCH. But it may equally well be simply an effect of excitation. The latter explanation requires that a large proportion of the CS Ca be doubly ionized in the hotter M stars, and it invalidates Deutsch's calculations of  $-\dot{M}$ . The absence of CS lines in giants earlier than M0 would then not preclude the occurrence of mass-loss from these objects. In fact, the ultraviolet continuum of a K giant is probably sufficient for double ionization of virtually all the Ca that may be in the envelope.

If we admit this possibility, we must also admit that invisible mass-loss may occur from the G and K giants at rates comparable with the rate of visible mass-loss in the late M giants. Considerations of stellar evolution suggest that this must be true. A star, which evolves from  $\Delta 0$  on the main sequence to M5, cannot remain a late M star long enough to lose the requisite amount of mass for transformation to a white dwarf. But its total lifetime on the red side of the Hertzsprung gap is amply long for the requisite mass-loss, if this proceeds uniformly at approximately the rate we observe in the late M giants.

### 3. - The spectra of two M-type supergiants.

Except for the observations we have just discussed of Ca II *H* and *K* and a few other zero-volt lines in the spectra of several score M giants, we have little information relating to the structure of the expanding envelopes around M giants. For two supergiants in which the CS spectrum is very well developed, more detailed studies have been made. These stars are  $\alpha$  *Herculis* and  $\alpha$  *Orionis*. Table II gives the values that have been adopted for the dimensions and other physical parameters of these stars. The distances and masses are not well determined, and could conceivably be wrong by factors of about two.

We shall first discuss the spectrum of  $\alpha$  *Herculis*, and the inferences that have been drawn from it regarding the structure of the atmosphere and the CS envelope. The reversing layer produces wide, shallow absorption lines, in which the central intensity increases systematically with excitation potential for lines of the same width. SPITZER (1939) finds that the line profiles indicate



microturbulence with a velocity spectrum that approximates a dispersion law. The fictitious damping constant  $\gamma$ , which gives the half-width of the line at half-intensity, is about 4 km/s.

TABLE II.

Spectral type	$\alpha$ <i>Herculis</i> M5 II	$\alpha$ <i>Orionis</i> M2 Ib
Distance (pc)	150	210
Mean absolute magnitude, $M_v$	-2.4	-5.7
Mean bolometric magnitude, $M_B$	-5.9	-9
Effective temperature, $T_E$ (°K)	2700	3150
Radius ( $\odot = 1$ )	580	1610
Mass ( $\odot = 1$ )	15	2)
Escape velocity, $V_{es}$ (km/s)	99	87
Thermal velocity at $T_E$ , $V_{th}$ (km/s)	7.0	7.5
Flow velocity, $\bar{V}$ (km/s)	10	8

A number of emission lines occur, which are attributed to a chromosphere, or region of inverted temperature gradient. The strongest of these lines are at Ca II *H* and *K* (see Fig. 2); others lie in the wings of *K* and in the far ultraviolet (HERZBERG, 1948).

The CS envelope produces violet-shifted absorption cores in all strong lines arising from the ground level. In addition, CS cores occur in the strongest lines that arise from excited sub-levels within the ground term; two examples are Al I 3962, with an excitation potential of 0.01 eV (see Fig. 2), and Fe I 3856, with an excitation potential of 0.05 eV. The central intensities of the CS lines are indistinguishable from zero when they are fully resolved on the spectrograms.

A similar CS spectrum is seen in the visual companion; however, all the CS lines are weaker there than in the M star, and the lines from excited sub-levels are completely absent. The excitation temperature is then very low, but not zero, near the M star; and it is indistinguishable from zero in the outer parts of the envelope which are traversed by the line of sight to the G star. Neither the CS line profiles nor their radial velocities give any evidence for a velocity gradient in the envelope, except that *H* and *K*, which are the strongest CS lines, yield an expansion velocity significantly smaller (by 4 km/s) than that of 10 km/s which characterizes the other CS lines.

In his 1956 discussion of the CS spectrum, DEUTSCH found the surface density of Ca II to be 28 times that of Ca I along the line of sight to the G star. This is a surprisingly low degree of ionization for so rarefied a gas as that in the outer envelope. Indeed, on the basis of certain reasonable assumptions regarding the geometry of the system and the source of ionizing radiation, DEUTSCH found it impossible to account for so low an ionization,

unless the gas were concentrated in clouds which fill the volume of the envelope with a packing fraction of only  $10^{-7}$ .

On the basis of new information, there is now reason to believe that this awkward requirement can be significantly relaxed. However, it is still not clear that one can understand the ionization without postulating some degree of lumpiness in the outer envelope. The phenomenon may be related to the one which maintains condensations in the high chromospheres of the super-giant stars which have been studied at chromospheric eclipses (WILSON, 1960b).

In the spectrum of  $\alpha$  *Orionis* one can see in a more exaggerated form all the same peculiarities that occur in  $\alpha$  *Herculis*. SPITZER (1939) finds that the reversing-layer lines have the profiles of dispersion curves, with the parameter  $\gamma = 8$  km/s. He also obtains from the spectrum a variety of conventionally defined «temperatures», the differences among which are taken to indicate very strong deviations from local thermodynamic equilibrium. These determinations range from an excitation temperature of  $2100^\circ$  for Fe I lines, to a kinetic temperature of  $200\,000^\circ$  for Fe atoms with  $(v^2)^{\frac{1}{2}} = 10$  km/s, corresponding to the observed dispersion profiles.

The CS spectrum of this star has recently been examined in detail by WEYMANN (1961). He adopts a model for the envelope in which the ionization temperature  $T_i = 2560^\circ$ , which is just the boundary temperature for a gray atmosphere with  $T_{\text{eff}} = 3150^\circ$ . With a total surface density of  $N = 1.2 \cdot 10^{22}$  atoms per  $\text{cm}^2$  in the envelope, he finds that double ionization is everywhere negligible. Moreover, without any condensations to inhibit ionization, the model yields CS line strengths which are in reasonable accord with the observations for the lines of neutral Al, Na, Ti, and Cr, and for the lines of singly-ionized Ca, Sc, Ti, Sr, and Ba. However, the model does predict CS lines which are too strong for Mn I and Fe I—a discrepancy which WEYMANN attributes to its neglect of an important contribution from the chromosphere to the ionization continuum of these atoms.

WEYMANN treats as a disposable parameter the radius  $r_0$  of the inner boundary of the observed envelope. Its value depends sensitively on the value of  $T_{\text{eff}}$ . He finds that  $r_0$  is about 11 times the stellar radius. At this height the flow velocity is still only 30% of the local escape velocity. The gas density is  $8 \cdot 10^{-16}$  g/cm<sup>3</sup>; the electron density 300 cm<sup>-3</sup>; and the kinetic temperature about  $1000^\circ$ . Collisional excitation in these conditions can then populate the fine-structure levels in agreement with the observations. The rate of mass-loss is  $2.6 \cdot 10^{20}$  g/s, or about ten times the rate of thermonuclear conversion of H to He in this star. For  $r > r_0$ , the density falls as  $r^{-2}$ ; the flow velocity  $V$  and the ionization remain constant. At large distances  $V$  also falls, as indicated by Ca II H and K. As in  $\alpha$  *Herculis*, the expansion velocity from these very strong lines is significantly smaller than from all the other CS lines.

On this model, we do not observe any CS gas below  $r_0$ . The gas must there-

fore be too hot there to contribute to the resonance lines seen in the CS spectrum. It is not yet clear what limits must be placed on the temperature, however, to insure that this region of the model will not yield emission lines which have no counterpart in the real star.

#### 4. — Spherically-symmetric flows in a gravitational field.

In order to assess the theoretical implications of the massive outflows observed in many stars, we shall now survey some of the properties of spherically-symmetric stationary flows in an inverse-square gravitational field. Several authors have discussed this problem from the point of view of coronal evaporation (RUBBRA and COWLING, 1960; CHAMBERLAIN, 1960; DE JAGER, 1960). The first of these papers has also given brief consideration to flows in which electromagnetic forces are important. WILSON (1960a) has explored the possibility that the envelope is expelled by forces arising from radiation pressure in Lyman  $\alpha$ . The theory is difficult because of the effects of non-coherent scattering; however, there can be no doubt that this mechanism requires an exceedingly high mean intensity of Lyman  $\alpha$  within the envelope. WEYMANN (1961) has concluded that in  $\alpha$  *Orionis* the requisite intensity of Lyman  $\alpha$  would doubly ionize Ba to an extent inconsistent with the observed strength of the line Ba II 4554.

In the present context, it is appropriate to assume that the mean-free-path of an atom or ion is sufficiently short so that a hydrodynamical description is a valid one. We shall also assume that the only forces acting on the gas are those due to the pressure gradient and to gravity. The Eulerian equation of motion may then be written as

$$(1) \quad V \frac{dV}{dr} = - \frac{1}{\varrho} \frac{dP}{dr} - \frac{GM}{r^2}.$$

Here  $V$ ,  $\varrho$ , and  $P$  are, respectively, the flow velocity, the density, and the gas pressure at distance  $r$  from the center of a star of mass  $M$ . We also have the continuity equation

$$(2) \quad 4\pi r^2 \varrho V = -\dot{M} = \text{const},$$

and the gas law (in the usual notation)

$$(3) \quad P = \frac{k}{\mu m_H} \varrho T.$$

We shall suppose that the gas is monatomic, with a ratio of specific heats  $c_p/c_v = \frac{5}{3}$ . We shall approximate the actual flows with the polytropic law

$$(4) \quad P \varrho^{-\gamma} = a = \text{const},$$

where  $\gamma$  may have any constant value in the range  $0 \leq \gamma < \frac{5}{3}$ . We are therefore not limited to the discussion of isentropic flows.

The integral of eq. (1) follows, using eq. (4), and is the equivalent of the Bernoulli equation. For  $\gamma \neq 1$ , it is

$$(5) \quad \frac{1}{2} V^2 - \frac{GM}{r} + \frac{P/\varrho}{1-1/\gamma} = K = \text{const},$$

and for  $\gamma = 1$ ,

$$(6) \quad \frac{1}{2} V^2 - \frac{GM}{r} + \frac{P}{\varrho} \ln P = K = \text{const}.$$

In recent years a number of authors have discussed these equations, and the associated equation for the transport of energy. BONDI (1952) has considered the problem in connection with the accretion of interstellar gas by continuous inflow to a star. MCCREA (1956) has generalized Bondi's results for the case where the inflow possesses a standing shock-wave. PARKER (1958, 1960*a*, 1960*b*) and CHAMBERLAIN (1960, 1961) have tried to represent the solar corona by different models of outflow based on the same equations. ROGERS (1956) and WEYMANN (1960) have attempted to apply them to the phenomena observed in the M-type supergiants. STANYUKOVICH (1959) has also studied these and similar equations in the context of various astrophysical processes.

In this paper we shall briefly examine some simple solutions for the outflow from a star of prescribed radius  $r_0$  and mass  $M$ . If we arbitrarily assign the polytropic exponent  $\gamma$ , and the values at the base of the flow of the velocity, density, and temperature, we may then derive from the equations given above the formal solutions for  $V(r)$ ,  $\varrho(r)$ ,  $T(r)$ , and  $P(r)$ . But included in the manifold of formal solutions obtained in this way, there will be many which are inadmissible in the present context. We shall limit ourselves here to the discussion of solutions which are single-valued and positive real between the base of the flow and infinity, and in which the acceleration of the gas remains finite. The first of these restrictions is a necessary consequence of the assumption in eq. (2) that the flow describes a single polytrope from  $r_0$  to  $\infty$ . The arbitrariness of this assumption should be kept in mind when we consider the consequences below.

It will be convenient for us to introduce the following notation. We let  $z$  be the distance in units of the stellar radius;  $c$  the local velocity of sound for



a monatomic gas;  $\beta$  the local Mach number of the flow; and  $\alpha$  proportional to the ratio of gravitational potential energy to thermal energy at the base of the flow. We also let the local escape velocity and thermal velocity be  $V_{es}$  and  $V_{th}$ , respectively. The subscript zero will always denote the base of the flow. We then have the following relations:

$$(7) \quad \begin{cases} \chi = r/r_0, & c^2 = \frac{5kT}{3\mu m_H}, & \beta = V/c, \\ \alpha = \frac{GM}{2r_0 c_0^2} = \frac{9}{20} (V_{es}/V_{th})_0^2. \end{cases}$$

For a star of given escape velocity  $(V_{es})_0$ , the specification of  $T_0$  and  $V_0$  is equivalent to the specification of  $\alpha$  and  $\beta_0$ .

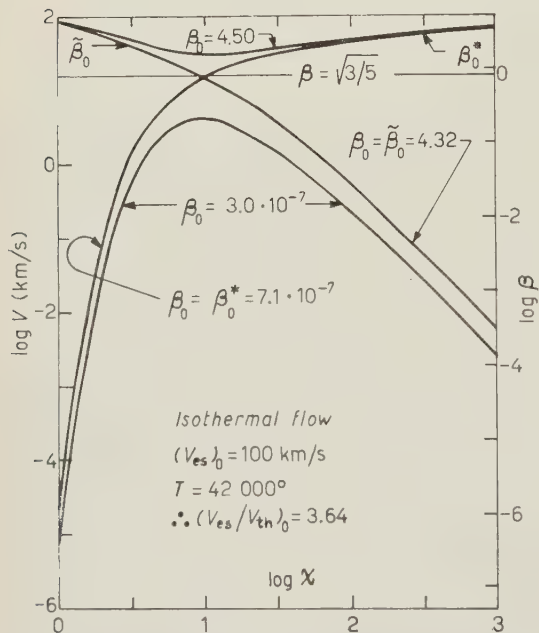


Fig. 6. — The flow velocity  $V$  as a function of  $\chi = r/r_0$ , in four representative isothermal flows. The subsonic flow and the decelerating transonic flow go to zero velocity as  $\chi \rightarrow \infty$ .

than  $\sqrt{\frac{3}{5}}$ ; it falls to a minimum at  $\chi = \frac{5}{3}\alpha$ , and at large distances increases as  $(\ln \chi)^{\frac{1}{2}}$ . The critical values of  $\beta_0$  which delimit these two regimes (see Fig. 7) are the zeros of eq. (8) when  $\chi = \frac{5}{3}\alpha$  and  $\beta = \sqrt{\frac{3}{5}}$ . Let us call these zeros  $\beta_0^*$  and  $\tilde{\beta}_0 = \beta_0^*$ . For  $\beta_0^* < \beta_0 < \tilde{\beta}_0$ , there is a discontinuity in the acceleration of the gas at the point where  $\beta = \sqrt{\frac{3}{5}}$ , and these solutions are inadmissible.

#### A) The isothermal case.

We shall first discuss the case of isothermal flow,  $\gamma = 1$ . When we solve for the local Mach number, we obtain the equation

$$(8) \quad \frac{1}{2}(\beta^2 - \beta_0^2) - \frac{3}{5} \ln(\beta/\beta_0) = \frac{6}{5} \ln \chi - 2\alpha(1 - 1/\chi).$$

The character of this solution depends on the values of  $\alpha$  and  $\beta_0$ . When  $\alpha \geq \frac{8}{5}$ , the velocity profiles are like one of the examples in Fig. 6. For  $\beta_0$  sufficiently small,  $\beta$  remains less than  $\sqrt{\frac{3}{5}}$ ; it rises to a maximum at  $\chi = \frac{5}{3}\alpha$ , and at large distance vanishes as  $\chi^{-2}$ . For  $\beta_0$  sufficiently large,  $\beta$  remains greater

For  $\beta_0 = \beta_0^*$ , a singular solution exists in which  $\beta$  increases monotonically with  $\chi$ ; the flow is subsonic for small  $\chi$  and supersonic for large  $\chi$ , with  $\beta$  diverging logarithmically as  $\chi \rightarrow \infty$ . For  $\beta_0 = \tilde{\beta}_0$ , a second singular solution exists, in which  $\beta$  decreases monotonically; the flow is supersonic for small  $\chi$  and subsonic for large  $\chi$ , with  $\beta$  going to zero with  $\chi^{-2}$ .

When  $\alpha < \frac{3}{5}$ ,  $\beta_0$  may take any positive value except  $\sqrt{\frac{3}{5}}$ . In these solutions,  $\beta$  decreases monotonically if  $\beta_0 < \sqrt{\frac{3}{5}}$ , and it increases monotonically if  $\beta_0 > \sqrt{\frac{3}{5}}$ . The asymptotic forms are the same as for the case where  $\alpha \geq \frac{3}{5}$ .

If we seek to approximate the observed outflows from M giants by these isothermal solutions, it seems appropriate to impose additional boundary conditions upon them which will insure a fit with the interstellar medium as  $\chi \rightarrow \infty$ .

In particular, we shall require that  $V \rightarrow 0$  and  $P \rightarrow P_I$ , where the subscript  $I$  denotes the interstellar medium. These far boundary conditions may

be satisfied only by the subsonic solutions, with  $\beta_0 < \beta^*$ ; and by the decelerated critical solution, with  $\beta_0 = \tilde{\beta}_0$ . In either of these cases, it may be shown that to produce a mass-loss ( $-\dot{M}$ ) we must have

$$(9) \quad \alpha \geq \left| \frac{10kT_I(-\dot{M})}{\pi(5c/3)^{\frac{3}{2}} \cdot 3\mu m_H r_0^2 (V_{es})_0^3 \varrho_I} \right|.$$

As representative of an M giant star in an H I region of the interstellar medium, we take  $r_0 = 100 R_\odot$ ,  $(V_{es})_0 = 100$  km/s,  $T_I = 200^\circ$ ,  $\varrho_I = 2 \cdot 10^{-25}$  g/cm<sup>3</sup>,  $-\dot{M} = 10^{11}$  g/s. We then find that  $\alpha = 1.75$ ; and, from eq. (7),  $T = 1400$ . Furthermore, we may show that for subsonic flows with  $\alpha > 1$ , we have

$$(10) \quad \varrho_0 \geq \frac{3\mu m_H (V_{es})_0^2 \varrho_I}{20kT_I} (5c/3)^{\frac{3}{2}} \alpha \exp \left| \frac{10}{3} \alpha \right|.$$

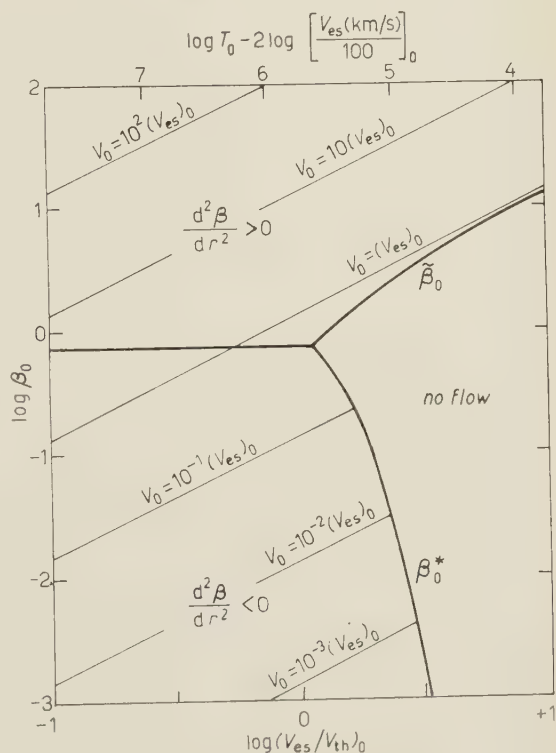


Fig. 7. — The regimes of isothermal flow. The scale at the top is drawn for  $\mu = 1.38$ .

With  $\alpha > 175$ , this becomes  $\varrho_0 > 10^{233} \text{ g/cm}^3$ ! This result reflects the circumstance, that, in a massive subsonic isothermal flow, the velocity must be extremely small near the base of the flow. It is clear that we cannot hope to represent the observed flows with such a model, regardless of any reasonable changes in the parameters of eq. (9) and (10) such as  $T_I$ ,  $r_0$ , etc.

Let us now consider the decelerating critical flow. In this case, we have

$$(11) \quad \varrho_0 \geq \frac{3\mu m_H (V_{es})_0^2 \varrho_I}{10kT_I} (5e/3)^{\frac{1}{2}} \alpha^{\frac{1}{2}} \geq 6 \cdot 10^{-20} \text{ g/cm}^3.$$

Thus, this flow cannot be ruled out on the grounds of too high a density near the star. However, it may be shown that, when  $\alpha \gg 1$  in this flow,

$$(12) \quad V_0 = \sqrt{2} (V_{es})_0.$$

In our example, therefore, the initial flow velocity would be 140 km/s, and this cannot be reconciled with the observations.

*B) The adiabatic case.* Putting  $\gamma = \frac{5}{3}$  for the adiabatic flow of a mono-

atomic gas, we find that the solution may be written in the form

$$(13) \quad \chi = S^{-1} [(\beta/\beta_0)^{\frac{2}{3}} - 4\alpha/\beta_0^2 + (3/\beta_0^2)(\beta_0/\beta)^{\frac{2}{3}}].$$

where

$$(14) \quad S = \frac{3 - 4\alpha}{\beta_0^2} + 1.$$

We again limit consideration to solutions which are single valued and positive real between  $\chi = 1$  and  $\infty$ , and in which the acceleration of the gas remains finite. These restrictions require that we have  $S \geq 0$ . Fig. 8 shows the various flow regimes. No transonic flows exist. When  $S = 0$ , the Mach

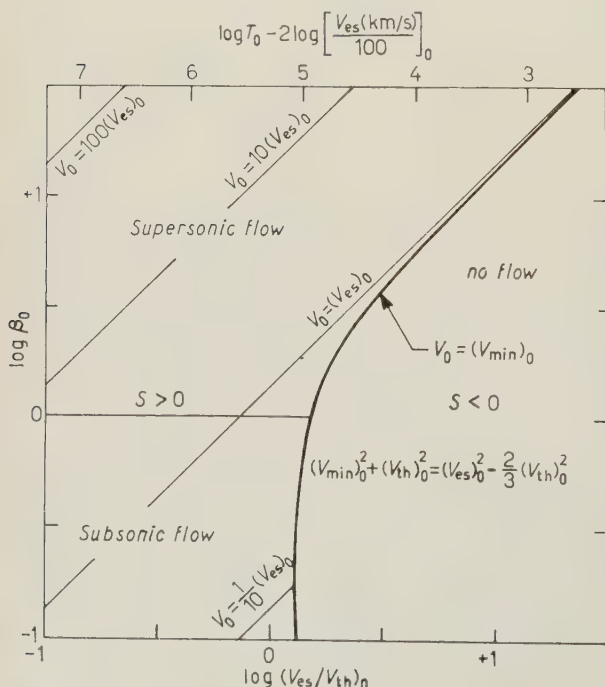


Fig. 8 - The regimes of adiabatic flow. The scale at the top is drawn for  $\mu = 0.5$ .

number remains constant throughout the flow. If  $S > 0$  and the flow is initially subsonic ( $\beta_0 < 1$ ), then  $\beta$  decreases monotonically and the flow remains subsonic; if the flow is initially supersonic ( $\beta_0 > 1$ ), then  $\beta$  increases monotonically, and the flow remains supersonic. If initially sonic ( $\beta_0 = 1$ ), the flow remains sonic ( $\beta \equiv 1$ ); this case can occur only when  $S = 0$ .

In flows with  $S = 0$ ,  $T$  and  $V$  both vanish as  $\chi \rightarrow \infty$ . Along the curve  $S = 0$ , then, the gas at the base of the flow has just enough energy to reach infinity. For given values of the escape velocity and the thermal velocity at the base of the flow, therefore, this limiting curve gives the corresponding minimum value of the initial flow velocity. We find that

$$(15) \quad \frac{1}{2}(V_{in})_0^2 + \frac{1}{2}(V_{th})_0^2 = \frac{1}{2}(V_{es})_0^2 - \frac{1}{2}(V_{th})_0^2.$$

In this form, the equation points up the fact that, although the flow is adiabatic, a unit mass of gas may move to infinity down the existing pressure gradient, even though its energy content would be insufficient for ballistic escape.

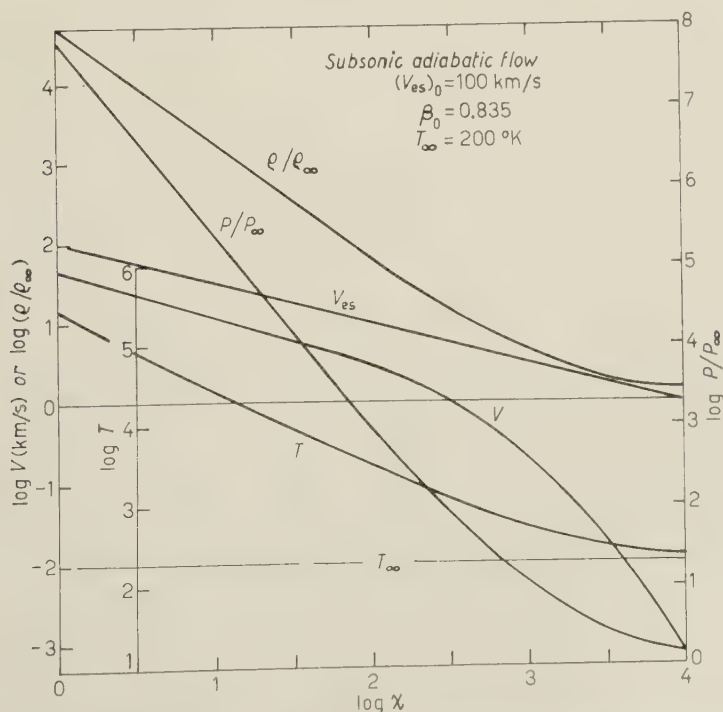


Fig. 9. A representative subsonic adiabatic flow, with  $\mu = 1.38$ . As  $\chi \rightarrow \infty$ ,  $V \rightarrow 0$ , and the temperature goes to the interstellar value,  $T_I = 2.0^\circ$ .



Some typical adiabatic flows are illustrated in Fig. 9 and 10. In the subsonic solutions  $V$  vanishes as  $\chi \rightarrow \infty$ ; in the supersonic solutions,  $V \rightarrow V_0 S^{\frac{1}{2}}$ . Conversely, the pressure goes to a finite limit in the subsonic solutions,

$$(16) \quad P \rightarrow P_0 \beta_0^5 (S/3)^{\frac{5}{2}} \quad \text{as} \quad \chi \rightarrow \infty,$$

and it vanishes in the supersonic solutions. If we require a fit with the interstellar medium, we must therefore exclude the supersonic solutions. With

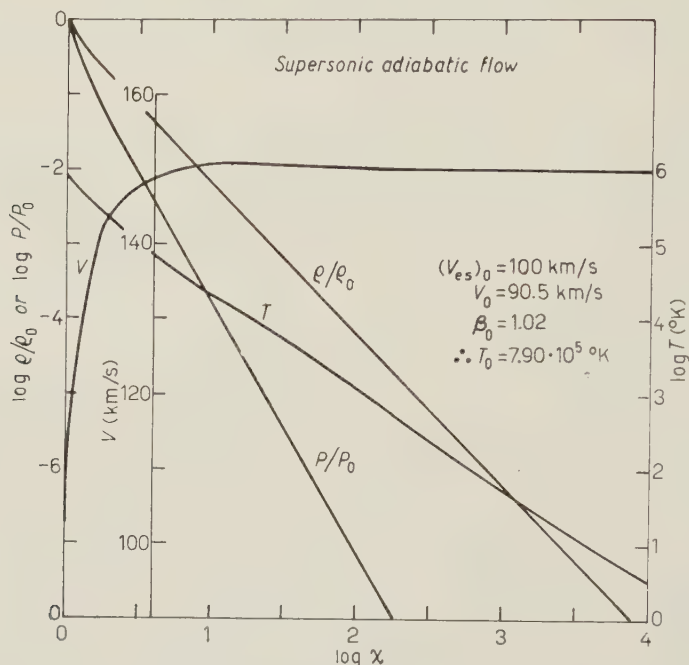


Fig. 10. — A representative supersonic adiabatic flow, with  $\mu=1.38$ . This does not fit to the interstellar medium.

the condition that  $P_\infty = P_I$ , the subsonic solutions yield a mass-loss given by the equation

$$(17) \quad -\dot{M} = 2\pi \left( \frac{3\mu m_H}{20k} \right)^{\frac{3}{2}} \alpha^{-1} \beta_0 r_0^2 (V_{es})_0^4 Q_{\infty}^{\frac{3}{2}} (T_I \rho_I)^{-\frac{1}{2}}.$$

It is possible to show that, over the relevant range of  $(V_{es})_0$  and  $T_\infty$ , we always have  $4\alpha \gg \beta_0^2 S$ . This leads to an appreciable simplification of the equations for subsonic adiabatic flow. In particular, if  $P_\infty = P_I$ , we find that the

initial conditions must satisfy the following equations, with  $\beta_0 < 1$ :

(18)

$$\left\{\begin{array}{l}V_0 = \frac{\beta_0}{(\beta_0^2 + 3)^{\frac{1}{2}}} (V_{\text{es}})_0, \\T_0 = \frac{(3\mu m_{\text{H}}/5k)}{\beta_0^2 + 3} (V_{\text{es}})_0^2, \\ \varrho_0 = \frac{(3\mu m_{\text{H}}/5k)^{\frac{3}{2}} \varrho_{\infty}^{\frac{1}{2}}}{(\beta_0^2 + 3)^{\frac{3}{2}} (\varrho_I T_I)^{\frac{1}{2}}} (V_{\text{es}})_0^3.\end{array}\right.$$

For the limiting cases  $\beta_0 = 0$  and 1, respectively, these relations are depicted in Fig. 11. It is notable that, for a given value of  $(V_{\text{es}})_0$ ,  $T_0$  and  $\varrho_0/\varrho_{\infty}$  are both confined within very narrow limits. We find the following values when  $\mu \simeq 1.38$ ,  $(V_{\text{es}})_0 \simeq 100$  km/s, as in the M giants; and  $\varrho_{\infty} = \varrho_I \simeq 2 \cdot 10^{-25}$  g/cm<sup>3</sup>,  $T_{\infty} = T_I \simeq 200^{\circ}$ , as in a typical H I region of the instellar medium:

(19)

$$\left\{\begin{array}{l}0 < \beta_0 < 1 \\3.3 \cdot 10^5 > T_0 \text{ (deg)} > 2.5 \cdot 10^5 \\2.4 \cdot 10^{-20} > \varrho_0 \text{ (g/cm}^3\text{)} > 1.8 \cdot 10^{-20} \\0 < V_0 \text{ (km/s)} < 50.\end{array}\right.$$

If such flows occur in M giants, they therefore endow these stars with structures which resemble the solar corona but are about ten times cooler.

In the solar corona itself, the flow must be very nearly adiabatic at sufficiently large distances from the sun. PARKER (1960*a, b*) has taken the position that in this

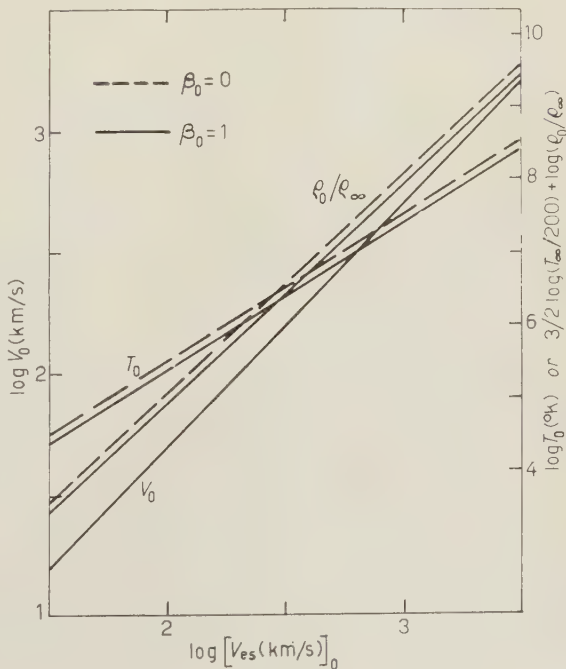


Fig. 11. – Initial conditions for adiabatic flows in which the pressure goes to the interstellar value at large distances. The initial velocity may lie anywhere below the line labeled  $V_0$ .

region the flow must be supersonic, for the reason that in the subsonic adiabatic solutions the pressure goes to a limit which greatly exceeds the interstellar value. This conclusion, however, appears to be the result of representing the isothermal part of the flow by the accelerating critical solution, and joining an adiabatic solution to this at a point where the flow is already supersonic. CHAMBERLAIN (1961) finds it possible to represent the outer corona with a subsonic adiabatic flow going to zero pressure at  $\infty$ .

If we apply eq. (18) to the sun, we also find it possible to represent the solar corona by a subsonic adiabatic flow going to any small value for the interstellar pressure. Thus, if we enter Fig. 11 with the value of  $(V_e)_0 = 308$  km/s, which is appropriate for the sun at a height of three solar radii above the photosphere, we obtain the following limits:

$$(20) \quad \left\{ \begin{array}{ll} 0 < \beta_0 & < 1 \\ 3.2 \cdot 10^6 > T_0 \text{ (deg)} & > 2.4 \cdot 10^6 \\ 3.9 \cdot 10^{-19} > \rho_0 \text{ (g/cm}^3\text{)} & > 2.6 \cdot 10^{-19} \\ 0 < V_0 \text{ (km/s)} & < 77 . \end{array} \right.$$

The limits computed for  $T_0$  and  $\rho_0$  lie within a factor of two or three of the actual values for  $T$  and  $\rho$  at this level in the solar corona. This relatively close agreement may well indicate that already within a few radii of the solar surface, the structure of the corona is largely determined by the requirement of a fit to the interstellar medium at large distances.

In contrast to the isothermal case, a massive subsonic adiabatic flow is always characterized by a very low density gradient. With the same values we have used before for an M giant in an H I region, we find from eq. (17) that  $-\dot{M} = 2.8 \cdot 10^{13}$  g/s when  $\alpha = \beta_0 = 1$ . This value can be appreciably increased only if  $\alpha \ll 1$ ; but then we find that  $(V_{th}/V_{es})_0 \gg 1$ , and  $\rho_\infty/\rho_0 \simeq 1$ . Moreover, eq. (17) shows that, for a star of given mass, these results are independent of the value taken for the radius  $r_0$  of the « base of the flow ». If the flows we observe are stationary, we may expect that at large distances they must become subsonic adiabatic. For these flows  $-\dot{M}$  is so large that we must have  $\alpha \ll 1$ . Therefore, the subsonic adiabatic regime can set in only at distances so large that  $\rho_0 \simeq \rho_\infty$ . The assumption of a steady state may therefore be untenable for these flows. WEYMANN (1960) has concluded that small non-steady effects in a non-adiabatic flow cannot substantially increase  $-\dot{M}$ , without violating the condition that the mass of the star must be at least comparably large with the mass of the envelope. Possibly it will be necessary to consider the motion of the star through the interstellar gas, in order to formulate the appropriate far boundary conditions. This motion will usually be supersonic. Possibly no theory for massive out-

flows can succeed unless it gives up one or both of the simplifying assumptions we have adopted, *viz.*, that the flow is stationary and spherically-symmetric.

In order to estimate the strength and radial velocity of the *K*-line that could be expected in a typical subsonic adiabatic flow, the projected surface density of Ca II ions was computed for the flow of Fig. 9. In order to take approximate account of the ionization of calcium, the following model was assumed. The star is surrounded by a sharply defined Strömgren sphere of ionized hydrogen. Outside of this, the metals are singly ionized on the average. Ca II occurs only outside the Strömgren sphere; its ionization corresponds to an ionization temperature  $T_i = 3000^\circ$ ; an electron temperature  $T_e$  equal to the local kinetic temperature  $T$  of Fig. 9; and the appropriate factor for geometrical dilution only. The fraction of calcium in the first state of ionization then rises from .084 at  $\log \chi = 0.5$  to .98 at  $\log \chi = 4.0$ . Taking the number density of atoms equal to  $0.1 \text{ cm}^{-3}$  in the interstellar gas, and integrating along the line of sight from  $\log \chi = 0.5$  to  $\log \chi = 4.0$ , we then find for the surface density of Ca II  $N_{\text{Ca II}} = 1 \cdot 10^{10} \text{ cm}^{-2}$ , and for the mean expansion velocity of these ions,  $\bar{V} = 3.7 \text{ km/s}$ . The computed surface density is therefore a hundred times lower than is observed at type M1, and the computed expansion velocity five times too low.

C) *Hydrodynamical effects of radiative cooling.* In the last two sections we have briefly described the properties of isothermal and adiabatic flows. ROGERS (1956), WEYMANN (1960), and PARKER (1961) have discussed other polytropic flows in which  $\gamma$  assumes a constant value in the range  $1 < \gamma < \frac{5}{3}$ . These other polytropic flows show many of the essential features we have noted in the cases where  $\gamma = 1$  and  $\frac{5}{3}$ , respectively. The case where  $\gamma = \frac{3}{2}$  is notable in providing, for a particular value of  $T_0$ , a singular solution in which the flow velocity remains strictly constant.

In any real flow the thermodynamic behavior of the gas may depend upon a variety of processes for energy transport, and the flow may therefore depart widely from a simple polytropic law. WEYMANN has discussed some of these transport mechanisms, and particularly the effects due to radiative cooling. As has been clearly pointed out by STANYUKOVICH, radiation losses produce distinctive hydrodynamical effects, depending upon whether the flow is subsonic or supersonic. Thus, by adducing the equation for the conservation of energy, one may obtain the relation

$$\frac{dV}{dr} = \frac{V(2c^2/r - GM/r^2) + (\gamma - 1)(dQ/dt)}{c^2(\beta^2 - 1)},$$

where  $dQ/dt$  is the net rate of heat-loss per gram of matter, and where the speed of sound  $c$  is here defined as  $(\gamma p/\rho)^{\frac{1}{2}}$ . In this form, the equation shows that, other things being equal, the heat loss decelerates a subsonic flow and



accelerates a supersonic flow. (The gravitational field clearly behaves in the opposite way.) The equation also indicates that the acceleration diverges at  $\beta=1$  in trans-sonic flows, except possibly in the special case where the term

$$\left[ V \left( \frac{2\phi^2}{r} - \frac{GM}{r^2} \right) + (\gamma - 1) \left( \frac{dQ}{dt} \right) \right],$$

vanishes with  $(\beta-1)$ .

On the basis of two extreme hypotheses about the atomic processes responsible for the radiation, WEYMANN has computed the radiative losses and their hydrodynamical effects in several cases of outflow. His calculations refer to an M-type supergiant, with  $r_0 = 580 R_\odot$  and  $M = 15 M_\odot$ . For initial values, he has taken  $10^5 < T_0 < 10^6$ ,  $10^{-17} < \rho_0 < 10^{-15}$ , and  $0.1 < \beta_0 < 1.5$ . He finds that the radiation drastically reduces the temperature below that corresponding to adiabatic flow: to one percent of the adiabatic temperature at  $\chi=1.1$ , in a typical example. Simultaneously, the subsonic flows are so sharply decelerated that the velocity falls to less than one percent of the adiabatic velocity in the same distance. At these densities, therefore, subsonic flows can be maintained only if the large radiative losses are compensated by a distributed heating mechanism. A flux of acoustic or magneto-hydrodynamic waves might provide the distributed energy source that is required. But WEYMANN finds substantial heating of this kind to be still required at very large distances from the star.

Radiative cooling has a similar effect upon the temperature distribution in the supersonic flows WEYMANN has considered; but in these flows the cooling sharply accelerates the gas. At distances larger than  $\chi \simeq 2$ , where the hydrogen has recombined and the radiative cooling has greatly decreased, the velocity falls below that for adiabatic flow, although the flow remains supersonic. Depending on  $T_0$  and  $\rho_0$ , this deceleration may be sufficient to stop the gas at a finite distance; or it may allow the gas to escape at high velocity; or it may allow the gas to escape at a velocity comparable with the low values that are observed. But in a case of the last kind, WEYMANN finds that despite the high temperatures near the star, most of the Ca I and Ca II along the line of sight lies in this region. These particles produce lines indicating an expansion velocity of about 95 km/s, instead of the 10 km/s that is observed

## REFERENCES

- ADAMS, W. S. and MACCORMACK, E., 1935, *Ap. J.*, **81**, 119.  
 ALLER, L. H., 1956, *Gaseous Nebulae* (New York).  
 BONDI, H., 1952, *M. N.*, **112**, 195.  
 BOYARCHUK, A. A., 1958, *Eighth Liège Symposium*, 159.

- BURBIDGE, E. and BURBIDGE, G., 1958, *Encyclopedia of Physics*, ed. S. FLÜGGE (Berlin), **51**, 134.
- CHAMBERLAIN, J. W., 1960, *Ap. J.*, **131**, 47.
- CHAMBERLAIN, J. W., 1961, *Ap. J.*, **133**, 675.
- DE JAGER, C. D., 1960, *Ninth Liège Symposium*, 280.
- DEUTSCH, A. J., 1956, *Ap. J.*, **123**, 210.
- DEUTSCH, A. J., 1960, *Stellar Atmospheres*, ed J. L. GREENSTEIN (Chicago), 543.
- GAPOSCHKIN, C. P., 1957, *The Galactic Novae* (Amsterdam).
- HERZBERG, G., 1948, *Ap. J.*, **107**, 94.
- MCCREA, W. H., 1956, *Ap. J.*, **124**, 461.
- PAGEL, B. E. J., 1958, *Eighth Liège Symposium*, 177.
- PARKER, E. N., 1958, *Ap. J.*, **128**, 664.
- PARKER, E. N., 1960*a*, *Ap. J.*, **132**, 175.
- PARKER, E. N., 1960*b*, *Ap. J.* **132**, 821.
- ROGERS, M. H., 1956, unpublished manuscript.
- RUBBRA, F. T. and COWLING, T. G., 1960, *Ninth Liège Symposium*, 274.
- SAHADE, J., 1960, *Stellar Atmospheres*, ed. J. L. GREENSTEIN (Chicago), 466.
- SANDAGE, A. R., 1957, *Ap. J.*, **125**, 422.
- SCHMIDT, M., 1959, *Ap. J.*, **129**, 243.
- SEATON, M. J., 1960, *Reports on Progress in Physics*, **23**, 313.
- SOBOLEV, V. V., 1960, *A.J.U.S.S.R.*, **36**, 753.
- SPITZER, L., Jr., 1939, *Ap. J.*, **90**, 494.
- STANYUKOVICH, K. P., 1958, *Vrednie v Kosmicheskuiu Gazodinamiku* (Moscow), Sec. 20.
- STRUVE, O., 1958, *Pub. A.S.P.*, **70**, 5.
- UNDERHILL, A. B., 1959, *Pub. Dominion Astrophysical Observatory*, **11**, 209.
- UNDERHILL, A. B., 1960, *Stellar Atmospheres*, ed. J. L. GREENSTEIN (Chicago), 411.
- UNDERHILL, A. B., 1961, these proceedings, p. 69.
- WEYMANN, R., 1960, *Ap. J.*, **132**, 380.
- WEYMANN, R., 1961, *Ap. J.* (in press).
- WILSON, O. C., 1960*a*, *Ap. J.*, **132**, 136.
- WILSON, O. C., 1960*b*, *Stellar Atmospheres*, ed. J. L. GREENSTEIN (Chicago), 436.

## PART III.

### Spherically-Symmetric Motions in Stellar Atmospheres.

#### C. - Non-Catastrophic Mass-loss from Stars.

---

##### I.

##### Discussion.

*Chairman:* C. DE JAGER

(*Ed. Note:* This discussion spread over one and a half days, mainly stemming from Deutsch's emphasis on possible stellar mass-loss at a steady flow less than the escape velocity, Parker's emphasis on the «solar wind» as the very high speed mass-ejection mechanism for the sun, and the feeling of the aerodynamicists that the problems could be combined and generalized as a simulation of the diverging-converging nozzle-flow problem. So in editing, parts of the discussion have been re-ordered, for greater continuity. Following the Chairman's original schedule, stellar problems come first, then solar. Two summarizing presentations from the nozzle-flow standpoint were presented—one by CLAUSER, one by GERMAIN. Germain's presentation is used as the initiating point of re-discussion of several aspects in this transcription; CLAUSER has expanded his remarks and published them as a contribution from the Johns Hopkins University Department of Mechanics—AFOSR TN 60-1386 November 1960, so they are not included here.)

— A. UNDERHILL:

I would like to point out some inconsistencies in Deutsch's table. For the M-type giants, DEUTSCH has reported some very beautiful and detailed observations from which one can deduce that the flow at large distances is larger than the escape velocity at the same place. Very sensitive and intricate interpretations allow one to infer somewhat similar facts for the sun. In the case of the stars, rather crude and general arguments are given; and if one examines Deutsch's figures for the Wolf-Rayet and Be-type stars, one sees that the flow velocity listed is in most cases somewhat smaller than the escape velocity. We have heard that arguments based on evolutionary considerations favor values for mass-losses of the order of 10 times larger than those derived from these observations. I believe that this factor of 10 can be made up quite

easily, simply by altering the distance from the stellar surface at which the material has motions, and the size of the motions themselves.

Take the case of the Wolf-Rayet star HD 192103, spectral class WC 7. As far as I can tell from spectroscopic observations, this star appears to have a single spectrum, and the He II lines  $\lambda$  4686,  $\lambda$  3203 (from level 3),  $\lambda$  5411,  $\lambda$  4541 (from level 4), and  $\lambda$  6527 (from level 5) all have the same total half-width, which in the case of this star is of the order of 1000 km/s: this implies a root-mean-square velocity of roughly  $(500 \div 600)$  km/s. These are all emission lines. In this star one observes strongly displaced He I absorption in the lines  $\lambda$  3888 and  $\lambda$  3186, which originate from the metastable  $2^3S$  level; these are the only strong absorption lines in the spectrum. Under conditions of high temperature and moderate geometric dilution (*i.e.* at a distance of the order of 5 stellar radii) this level becomes strongly populated. A simple uniformly expanding sphere cannot be used to explain the He II profiles, one of the difficulties with the interpretation of Wolf-Rayet spectra being that the part of a strong line projected on the disk should be self-reversed, upon this theory, thus causing absorption on the violet side of the line, but in fact no such absorption is observed. The significance of this was recognized 30 years ago. Thus, it is significant that strong absorption only occurs for spectral lines which are strengthened when the material is far from the stellar surface, and that these lines show velocities of expansion of the order of 1200 km/s. I think the intrinsically strong line  $\lambda$  4686 does have some self-absorption (as it should have, since it's formed in a moderately dense region), but this self-absorption is such that it is all over the profile.

So the picture is that the Wolf-Rayet stars are composed of an inner atmosphere, which forms the emission spectrum chiefly, and an outer region at quite a distance from the star, which usually doesn't possess enough material to produce emission features, although it does produce certain absorption lines. This outer shell gives observational evidence for a definite expansion.

I repeat that most of the emission features in a Wolf-Rayet spectrum come from parts of the atmosphere where the motion is not uniformly directed. For example, the line  $\lambda$  5696 of C III has the only flat-topped profile which I have observed in a Wolf-Rayet spectrum to date, and there is no doubt that in HD 192103 this profile has a width of close to 2000 km/s, whereas the strong He I lines in this star have widths of no more than about 1000 km/s. So I am saying that an expanding envelope exists outside the regular Wolf-Rayet atmosphere. I think that you can also infer this from some of the binaries.

— G. ELSTE:

May there be a strong self-absorption at the C III  $\lambda$  5696 producing the flat-top?



— A. UNDERHILL:

I do not think so. This line is intrinsically a moderately weak line. I think that if there was going to be self-absorption, you would see it first of all in C III 4650 — you do not.

— E. SPIEGEL:

Could you just remind us of the evidence from the Wolf-Rayet stars that are members of binaries, whether it is the case that the shell would be near the critical zero velocity surface, and can one therefore get an idea of the consistency of these ideas of the shell sizes?

— A. UNDERHILL:

I do not remember offhand the sizes of the orbit and stars. What I remember about *V444 Cygni* is that the major part of the emission lines must be formed reasonably close to the stellar surface. You see you have a light curve and you have an eclipse; to interpret the details, you must postulate a fairly small nucleus which gives your continuous spectrum—another region that is reasonably dense—and then an extremely large region in which electron scattering dominates. Now I think this outer region probably produces these special features I have been describing.

— Mrs. BURBIDGE:

I would like to make a quite different argument for the existence of mass-loss in WR stars and in particular this star *V444 Cygni*. If the WR stars are massive stars, one might argue from the appearance of their spectrum that they appear to be at a late evolutionary stage. Some WR stars have strong carbon and some strong nitrogen in their spectra. Although it is difficult to say anything about real abundances in the atmospheres, because obviously departures from LTE are very important in atmospheres like this, yet the existence of carbon and nitrogen fit in well with our ideas of nucleogenesis in the interior of stars and stellar evolution. If a massive star has radiated long enough for the hydrogen in its interior to be converted to helium, and the helium core has reached quite a size, we can then get high enough temperature and density in the center for the  $3\alpha$  reaction to be triggered.  $3\ ^4\text{He} \rightarrow\ ^{12}\text{C}$ , 3 helium nuclei go to carbon 12 so one would get some carbon in the interior. Now if there has been some mixing to the surface, we have two possibilities. If the carbon came straight out to the surface, one would have evidence of excess carbon on the surface of the star. Alternatively, if the carbon went through a hydrogen-burning shell, that is, a region where hydrogen is being converted to helium by the carbon-nitrogen-oxygen cycle, then the carbon would largely be converted to nitrogen and one would have a high abundance

of nitrogen on the surface. Now in *V444 Cygni*, the mass that was determined from the orbit was I think something like 10 solar masses for the WR component, whereas its companion, the O-type star, has a somewhat greater mass—I do not remember the figures but something on the order of 25 solar masses, I believe. Now if the WR star has reached a late evolutionary stage, while the O-type star is still on the Main sequence, how does it come about that the WR star has a lower mass than the O star? Because as DEUTSCH pointed out, the rate of consuming available fuel goes with the 2.5 power of the mass. So, I would just like to put this as a suggestion that at least one member of this binary star has lost a large amount of mass, about 15 solar masses or more in this case.

— C. DE JAGER:

There is really little *direct* evidence that WR stars are surely very old.

— J.-C. PECKER:

There is a good argument in favor of the mass loss of the WR star: the Lagrangian point falls right in the double system *V444 Cygni* at the outer limit of the shell of the WR component.

— B. E. J. PAGEL:

I do not think that it has been mentioned today that essentially there are two kinds of WR stars—at least there are two kinds of stars that show this characteristic spectrum with very broad emission lines. There are the ones that have been discussed this morning, and others which are the cores of certain planetary nebulae and which have a somewhat similar spectrum to the WR stars but probably have a considerably smaller radius, perhaps less than one solar radius. So I should like to ask whether the two groups might be considered to fit in any evolutionary scheme?

— A. UNDERHILL:

I think that the WR stars are considered to be Population I, certainly a good many of them are associated with O and B stars, which everybody considers to be Population I, that is young stars formed in spiral arms. Perhaps DEUTSCH could correct me, but are not the planetary nebulae and therefore the central stars considered to be Population II, an entirely different type of star?

— A. J. DEUTSCH:

I think the answer is yes. In putting this material on the board, I felt it was necessary to make some comments about the present ideas relating to

stellar evolution. But I think some of this material may be a bit irrelevant to the subject matter of principal interest here, and to the extent that evolutionary questions do not bear directly upon the hydrodynamical problems perhaps you would see fit to dismiss them for the time being and concentrate on the others.

— M. J. SEATON:

I would like to raise some questions concerning where the planetary nuclei fit into this scheme. My first comment is that DEUTSCH referred to the supernovae and the planetary nebulae as both catastrophic cases, and drew a line before going on to WR stars. There I think some distinctions should be made. The super-novae are catastrophic in the most literal sense; presumably the star is completely destroyed. On the other hand, it may be that only one tenth of the mass of a star forms a planetary nebula and that is not catastrophic in the sense of complete destruction of the star. Then one might ask: Is the formation of the planetary nebulae catastrophic in the sense that it is something that takes place suddenly, in the same way that a nova ejects a shell in a more or less violent outburst? Now there are good reasons for believing that the planetary nebulae are not just remnants of novae, but it is often thought that planetaries originate in a sudden outburst. Naturally the question arises as whether such an event has ever been observed. Have we ever seen something rather like a nova that is subsequently identified as a planetary nebula? Of course, one might try to answer this just by taking statistics. We do not observe very many planetary nebulae and we could ask if it is likely that we would have observed one in the stage of sudden formation? I think on the other hand, we have to question whether in fact one must think of the planetary nebulae as being catastrophic events in this sense. It has already just been mentioned that the nuclei of some of the planetary nebulae are very similar to WR stars and these appear to have certain fairly steady rates of ejection. I must, therefore, ask whether perhaps the planetary nebulae might not result from some steady ejection process.

The second question that I would like to raise is where the planetary nuclei fit in the scheme of classification. Now, of course, planetary nebulae are by no means all the same, they cover quite a range of degrees of excitation. Central star temperatures may be determined by a variety of methods all essentially due to ZANSTRA. For low excitation nebulae one obtains  $T_e \simeq 5 \cdot 10^4$  degrees and this is the sort of value that one would expect for an O star. On the other hand, if one takes really high excitation planetaries with very strong He II lines, then there is no doubt that star temperatures come at least as high as  $25 \cdot 10^4$  and now, of course, we are right outside of the normal range of spectral classification. For these quite extraordinary stars, I might just say it appears that their radii are fairly small, perhaps one tenth of the solar

radius. I would like to pose two questions. First, are there such stars that do not have planetary nebulae? One can observe them and tell something about their properties when they have a nebula associated with them, but the central stars themselves are very faint and insignificant objects. Possibly they only exist in association with nebulae. This brings me to my second question: Does a star which is as hot as  $25 \cdot 10^4$  degrees necessarily eject matter and form a nebula?

— C. DE JAGER:

I agree with DEUTSCH that we should not go too far into the problem of evolution, but on the other hand the problem of mass-loss is intimately connected with the evolution, so we do have sometimes to treat the evolution problems. I think it was VORONTSHOV-VELIAMINOV who for the first time suggested that planetary nebulae would go over into a novae and then finally to a white dwarf. This suggestion was based on their place in the Hertzsprung-Russell diagram and on the fact that these types of stars lose mass and so finally can arrive at a mass so low that they can go into a white dwarf. Does anybody know what the lifetime of a planetary nebula is?

— A. J. DEUTSCH:

Ten thousand years. It is my understanding that a planetary nebula is a one-shot affair. A star reaches a certain stage in its evolution when it releases one tenth of its mass, which then goes out into the interstellar medium. I don't know how long it takes the one tenth of a solar mass to flow out through a sphere drawn around the star just before this outburst takes place. It may take a year; I suspect it takes some tens of years as SEATON has suggested, maybe even quite a bit longer than this. But it happens only once; it gets rid of a tenth of a solar mass and then I believe it stops. It doesn't go on.

— R. N. THOMAS:

Would you tell us how you know? Are the ten thousand year life and the one tenth solar mass purely theoretical figures?

— A. J. DEUTSCH:

I believe that these numbers come from the following arguments. One measures the surface brightness of a planetary nebula, one knows its linear dimensions; one can, therefore, compute the total amount of mass in it. Result: One tenth of a solar mass. One also knows that rate at which the nebula is expanding, one then asks how long will it take before this will no longer be observable. Result: 10 000 years, roughly. I would also like to re-



mark on the point that was already made; *viz.*, there are good and sufficient astronomical reasons for distinguishing between the nuclei of the planetary nebulae and the classical Wolf-Rayet stars. The latter objects have luminosities of the order of a few thousand times that of the sun. The central stars of the planetary nebulae have luminosities which are comparable with the luminosity of the sun. In addition their kinematics and distribution in the galaxy are totally different. So, I think one cannot admit the possibility that the classical WR stars are about to become the nuclei of typical planetary nebulae. It is indeed a fact that at least two WR stars are known to have nebulae around them. The nebulae can actually be photographed against the sky. But, in neither case is the nebula at all typical planetary.

— M. J. SEATON:

I was not suggesting at all that one should bracket together the WR stars and planetary nuclei and think of them as being the same sort of object, but I do think that we should consider whether the type of steady ejection process in the WR stars is also the type of process taking place in the planetary nuclei.

I only comment on the numbers. If we take  $10^{-6} M_{\odot}/\text{year}$  as the ejection rate for a WR star, and a time of  $10^4$  years, we have  $10^{-2} M_{\odot}$  ejected, which is not much less than the figure of  $10^{-1} M_{\odot}$  given for planetary nebulae.

— E. N. PARKER:

To what extent is the matter in a planetary nebula composed of interstellar material swept up by the 15 km/s expansion velocity? If a large fraction is interstellar matter, then the present expansion velocity may be very much lower than the initial expansion velocity.

— M. J. SEATON:

The material could originate from interstellar matter only for nebulae which are thin shells; this could not be the case when the density is high throughout a large volume. Let me raise another point. This morning DEUTSCH presented us with the result that the electron temperature would have to be high to get ejection of matter; and if I understand correctly this is essentially a question of having enough velocity to exceed the escape velocity. On the other hand, we are very accustomed to thinking, for shells of stars and gaseous nebulae, that the kinetic temperature is about  $10^4$  °K. This is a problem to which UNSÖLD referred the other day in his remarks on the microscopic treatment of equilibrium phenomena. The temperature of  $10^4$  °K is determined by the  $O^{2+}$  ion. Also, the  $O^{2+}$  ion by its forbidden line emission provides a means of measuring the temperature; and the measured value is indeed just what one predicts from the theory of the thermal balance. But the problem may be more complicated.

Consider a nebula containing hydrogen, helium, and oxygen. We have the three regions shown in the figure. The boundary between  $O^{2+}$  and  $O^{3+}$  coincides with the boundary between  $He^+$  and  $He^{2+}$  because  $O^{2+}$  and  $He^+$  happen to have identical ionization potentials of 54.4 eV. The usual theory—and the (O III) measurements—apply only to the region containing  $O^{2+}$ . In the inner region, that containing  $He^{2+}$ , the temperature may be much higher. This is not only because there is no cooling by  $O^{2+}$ . The mechanism may be summarized as follows: In the inner region we are skimming off the really high-energy quanta of the central star, those with energies above 54.4 eV. Each of these quanta produce one quantum in the He II Lyman  $\alpha$  line, and this in turn ionizes a hydrogen atom and gives an electron with a kinetic energy of 27.2 eV; thus, for each quantum absorbed one gains at least 27.2 eV of kinetic energy.

From this it may be shown that the kinetic temperature will be of the order  $10^5$  °K. The size of this inner region depends on the temperature of the star, but its high kinetic temperature is quite insensitive to the star temperature. I would like to make one further remark which I am sure is not relevant but which concerns a problem discussed at previous symposia. At the meeting in Cambridge, England, ZANSTRA suggested that condensations in nebulae might result from the rather curious equation of state which one has with the  $O^{2+}$  cooling mechanism. The idea was that dense regions would be cooler since there would be an increased amount of  $O^{2+}$  due to recombination of  $O^{3+}$ . For thick nebulae one may expect the picture drawn above to be correct. The  $O^{2+}$  and  $O^{3+}$  will then be sharply separated in space and Zanstra's condensation mechanism will not work.

$$\begin{array}{c} \text{Star} \\ * \end{array} \quad \begin{array}{c} H^+ \\ He^{2+} \\ O^{3+}, O^{4+}, \dots \\ T_e \sim 10^5 \end{array} \left( \begin{array}{c} H^+ \\ He^+ \\ O^{2+}, O^+ \\ T_e \sim 10^4 \end{array} \right) \begin{array}{c} H^0 \\ He^0 \\ O^0 \end{array}$$

— C. DE JAGER:

I wonder if Mrs. BÖHM-VITENSE could come back to a point raised by her at the end of the discussion on pulsating variable stars. That is this: At a certain state during the pulsation or evolution of a star there may occur a local region where the effective gravity becomes negative. My question is whether this local region might become so extended in certain stars that it could be of importance for the mass-loss of a star.

— E. BÖHM-VITENSE:

I really do not think it could, because this unstable region will always first occur in the layer with a temperature around ten thousand degrees. Now

if the higher layers should become unstable—that means gas could flow out of the star—one could expect at least also the deeper layers to be unstable at the same time. That means that if the gas should be able to flow out at all, this should require an extended region that would flow out, and the star certainly could not exist very long in this state. I guess it would not even be formed. The question then is whether during the evolution of the star, the star could pass through a region where this whole layer could become unstable. This could happen if the product of the absorption coefficient,  $\kappa$ , times the radiation flux,  $F$ , which is proportional to the radiative acceleration, would be increased during evolution. Either  $\kappa$  or  $F$  could be raised.  $\kappa$  could be increased by increasing the pressure, but then the radiative acceleration would become important first in the deeper layers, and the only result would be that the atmosphere would be blown up slightly and the pressure would decrease again until there would be equilibrium restored. The same would happen if the flux would increase—that means if the effective temperatures would increase during evolution—then again the radiation would first make the layers around  $10\,000^\circ$  unstable, and would blow up this part of the atmosphere, and thereby lower the pressure and the  $\kappa$  in this region until the atmosphere is stable again. The outer layers of the atmosphere would remain stable during this process.

— E. SCHATZMAN:

The question is related to the problem of generation of planetary nebulae and WR stars following the line suggested by SHKLOVSKY. When in the course of evolution a star of large mass—I do not know precisely which mass—has developed an isothermal core of largest possible value, which is about 15% of the mass of the star, then the core starts contracting. But for such stars of such mass the contraction of the core is very fast, and the radiation which is generated in that central region of the star starts pushing away the mass of the outer layers. So we have the equivalent of a piston; a shock develops and this problem can be studied from the point of view of hydrodynamics. It should be possible to find how this shock-wave develops, and how any mass outflow is initiated. In such a case we might expect that the difference in the WR stars and the planetary nebulae is a difference 1) in chemical composition and 2) in mass. Especially in the WR stars the amount of energy available suffices to push away a large mass. I compute the amount of gravitational energy available in the gravitational core as being about  $10^{50}$  erg. Thus, this could push away, at  $10^8$  cm/s, about 10 solar masses.

— G. BURBIDGE:

Perhaps you can answer a question on the hypothesis of SHKLOVSKY. I have never been able to understand what physical process starts this thing

going. One starts with the star in an equilibrium configuration and then suddenly all the radiation is absorbed and generates momentum in the outer shell. In the extreme case this would suggest that the outer shell would heat up and expand, while moving outward, but the character of the radiation that we see would change.

— E. BÖHM-VITENSE:

It seems that this hypothesis uses the same mechanism discussed before—radiation pressure. I do not think it would result in strong mass ejection, because it would just expand the star a little bit until the star would be in equilibrium again, and nothing more would happen.

— W. H. MCCREA;

Whenever you see in physics anything being thrown away from a system, it results from energy becoming concentrated into small localities, as in spray from a breaking wave. How can this apply to stars? Is there no way of concentrating energy into small bits of the atmosphere of a star, so that we get material « spraying off » rather than « flowing » off?

— A. UNDERHILL:

General considerations suggest that Be stars lose mass at a rate of  $10^{-7}$  solar mass per year. Many people would get extremely happy if you make the Be stars live any more than  $10^7$  years. So  $10^7$  times  $10^{-7}$  means a loss of one solar mass in the lifetime of the star. For an average Be star the mass is  $5M_{\odot}$ . Thus, a larger rate of mass-loss than estimated is required if these stars are to be able to evolve to white dwarfs.

— C. DE JAGER:

If the sun was as far from us as a star, we would not infer any mass-loss at all. Still, it loses mass—and apparently at 500 km/s. Who knows what happens in B and Be stars?

— A. UNDERHILL:

This is my point—I think all numbers have been probably greatly underestimated. One more point. In trying to understand the planetary nebulae and the shells around the WR stars and Be stars, I sometimes wonder how much magnetic fields have to do with the observed phenomena. I understand that a magnetic field can keep material trapped near a star. It can also do something to throw material out. I would like to note the perhaps significant fact that the hot stars, the O's and B's and WR stars are supposed to be formed in fairly recent astronomical times in spiral arms where there are supposed to be magnetic fields. How can you lose the magnetic field? If you bring



gas that was in an interstellar field together to make a star, don't you have to bring a magnetic field with it? Then you will have stars with magnetic fields. Now, there is no possibility of observing these fields directly and in this way proving that there is a magnetic field. There is nothing that you can observe to show the field, but if it is there, won't it affect our ideas of mass-loss or ejection seriously?

— S. S. HUANG:

I would like to mention some work (cf. *Ann. d'Ap.*, 1959, **22**, 527) which I did recently which has some bearing on the present topic of mass-loss by a star. It is an observational result that in  $\alpha$  *Virginis* and in other B-type spectroscopic binaries having two sets of spectral lines, the secondaries, *i.e.* the less massive and less luminous components, are always overluminous with respect to their masses. Why should they be overluminous? We can have four possible explanations: 1) A result of evolution. Since a more massive star evolves faster than a less massive star, one would expect that the primary component should first depart from the main sequence and become abnormally luminous. Actually it is the secondary component that is overluminous. Therefore evolution is not an explanation. 2) A result of hydrodynamic flow of matter and consequent transport of energy from the primary to the secondary component. But the two components in these binaries are on the average of 10 stellar radii apart. They are not in physical contact. Indeed both stars are much smaller than the two lobes of the critical surface which limits the sizes of both components. 3) A result of energy transfer from the primary to the secondary through electromagnetic radiation. You can rule this out immediately because the amount of added energy is simply not large enough to account for the excess energy that the secondary components of these binaries radiate. 4) A result of energy transfer by corpuscular radiation. This is my final conclusion. I assume that the excess energy radiated away is transported from the primary to the secondary component through an exchange of high-energy corpuscles. It is proposed that there exists a common envelope, which may be regarded as a common corona, around the two components of a binary system. If the density of the common corona is higher than that found in the solar corona, a plausible assumption in view of the larger masses of the component stars—the overluminous nature of the secondary component can be satisfactorily explained.

It is interesting to recall that after this work was completed, a group of physicists including KUPPERIAN and others, then in the Naval Research Laboratory and now in Goddard Space Flight Center, NASA, found in rocket flights ultraviolet radiation coming from a nebula near to  $\alpha$  *Virginis*. The nebula is much larger than the common corona proposed in my papers. However, SHKLOVSKY has since proposed that the energy radiated in the ultra-

violet by the nebula comes from the corpuscular radiation emitted by  $\alpha$  *Virginis*.

I would like also to make a comment on the problem of hydrodynamic flow from stars. According to DEUTSCH, the hydrodynamic flow velocity observed in many stars is of the order of 10 km/s, which is less than one tenth of the escape velocity. How could we derive from this empirical result the conclusion that these stars are losing mass by hydrodynamic flow? Thus, it appears to me that except for novae, novalike objects, and perhaps planetary nebulae the observations are not strong enough to draw any conclusion concerning mass-loss through hydrodynamic flow.

— K. H. PRENDERGAST:

I have been asked to give an account of a theoretical investigation of gas flow in the neighborhood of a close binary system. I'll try to sketch this very quickly, and also indicate why I think it is exceedingly difficult to say anything about mass-loss from such considerations. Suppose we have two stars which move around one another in circular orbits at constant angular velocity—and also suppose that the radius of one of the stars (or possibly both) is comparable to the separation of the centers of the stars: this is what I mean by a close binary system. There exist systems in which there is gas not only in the atmospheres of the two stars, but also at a considerable distance *above* the atmospheres. Can we say anything about the motion of this gas? We have to ask two questions at the outset. First of all, how does the gas get out of the stars into the system (this I am not going to try to answer), and second what forces act on the gas once it has been removed from the atmosphere of one or the other of the stars. If we assume that the velocity field does not depend on time, the equations of motion contain the inertial term  $\mathbf{V} \cdot \text{grad } \mathbf{V}$ , the Coriolis force,  $2\boldsymbol{\omega} \times \mathbf{V}$ , the centrifugal force  $\boldsymbol{\omega} \times (\boldsymbol{\omega} \times \mathbf{r})$ , the gravitational attraction of the two stars, and the pressure gradient. But what is the effective pressure? There is, of course, the gas-kinetic pressure, but there may also be important effects due to radiation pressure, or magnetic pressure, and there could very easily be significant Reynold stresses. It is impossible to consider all of these, and I have chosen to discuss the equations neglecting the pressure terms entirely. We can offer the following excuse for this procedure: The contribution of the pressure terms to the equations of motion is of the order  $V^2/R$ , where  $V$  is a small-scale velocity (whether thermal or «turbulent» does not matter), and  $R$  is the distance from an element of gas to the center of mass of the system. If this term is to be comparable to the gravitational forces,  $V$  must be of the order of a few hundred kilometer/second, and there is no observational evidence for the existence of such small-scale, high velocities.

I now construct the «gradient-wind» approximation to the solution of the equations of motion with the pressure term neglected. The velocities com-

puted in this approximation are of the same order of magnitude as those indicated by the observations, and the flow pattern looks like the pictures that the observers have been drawing for a number of years. (Such pictures can be found in Struve's «Stellar Evolution», and a number of original papers as well.) It should be clear that these considerations have no bearing on the problem of mass-loss from close binary systems. In order to discuss mass-loss we would have to be able to follow the history of elements of gas ejected at arbitrary speeds, in arbitrary directions from various points on the surfaces of the stars. This is currently impossible for two reasons. In the first place, we do not have a physical theory which enables us to compute the effective pressure, and therefore we cannot write down the correct system of equations for the flow. Secondly, even if we knew the equations, it would be very difficult to integrate them, even on a big computer. An account of this work has appeared in the *Ap. J.*, **132**, 162 (1960).

— R. N. THOMAS:

Let me return to the  $H_\alpha$  profiles DEUTSCH had on the board for the Be stars—the profiles consisting of a broad emission on which, not necessarily symmetrically located, is apparently a self-reversal. If I understand correctly the arguments, they are that the self-reversal represents the emission from that portion of the shell lying between the observer and the main stellar disk, the shell being large in extent compared with the radius of the main disk, and there being some kind of radial expansion of the shell. Thus, the geometrical effect gives an emission which can be regarded as either absorption in a cooler shell; or if the shell is not cooler, as coming anyway from only a relatively few atoms emitting at the maximum velocity of expansion (or even contraction), so the apparent self-reversal occurs. But consider the similar appearance of other types of lines, showing a central emission core, with a self-reversal superposed on this core. For example, Olin Wilson's observations of the later type giants, where  $\text{Ca}^+ H$  and  $K$  lines show this behavior. Or, Lyman  $\alpha$  of H and the  $\text{Mg}^+$  and  $\text{Ca}^+$  lines in the sun. In these solar cases, where we know there is no question of a large shell, our suggested interpretation has been based on a simple solution of the transfer problem for a non-LTE source-function in an optically-thick, hot chromosphere. We can predict the essential observed features, with the separation of the emission peaks being a combined function of the details of the temperature gradient in the atmosphere and the Doppler width of the absorption coefficient. In the case of Wilson's observations of the  $H$  and  $K$  lines, the several suggested interpretations again do not involve an extended atmosphere. The question of the detailed profile is presently controversial; some make an interpretation only in terms of the effect of turbulence on the absorption coefficient profile; JEFFERIES and I introduce also the effect of variation of a non-LTE source-function. However, in the case

of the Be stars, it seems to be assumed that one must have a greatly extended atmosphere, in expansion, with the details of line formation essentially irrelevant. So I want simply to raise the question—how certain is it, that one really only needs such an extended atmosphere. Are you sure that such models as used for the sun, and the  $\text{Ca}^+$  lines observed by WILSON in the giants, need not be invoked?

— G. BURBIDGE:

I have no immediate answer, except to say try it and see if you can get the correct profile

— A. UNDERHILL:

A general remark on observational problems in astrophysics is that observations of one feature only permits several interpretations. For these stars, my preference for the present model—and the self-reversal as absorption by the narrow region in front of the disk—is based on its bringing together quite a few pieces of observation

— Mrs. BURBIDGE.

We used to wonder whether the emission lines in the Be stars could be produced in some way other than in a ring around the star. But when one computes the number of emitting atoms—*e.g.* consider the great strength of the  $H_\alpha$  emission—the level of the emission line is way above the continuum—it seems to us that it is the great number of emitting atoms that makes an extended atmosphere seem necessary.

— R. N. THOMAS:

There is no problem about the absolute emission in a line relative to the continuum—once there are enough atoms to provide an opaque chromosphere, the intensity is a function of the size and position of the temperature rise in the outer atmosphere. And, once I get an emission line in such an atmosphere, I get a self-reversed core, except for very unusual circumstance of temperature gradient.

— A. UNDERHILL:

Be stars do not have a temperature increasing outward.

— A. J. DEUTSCH:

I want to endorse the comment of A. UNDERHILL. One has to look at the spectrum of a Be star as an entity, and he then sees that, in addition to  $H_\alpha$ , there often exist other absorption features, in many of these stars, which obviously are produced in an extended shell. There are absorption lines which



arise from metastable levels only, and are therefore characteristic of a dilute radiation field; so that one knows the star in this case to be in fact surrounded by an extended envelope. Now it must be added that for some Be stars, notably among the giants, one does not have this kind of evidence, and there is reason to believe that an explanation in terms of a temperature reversal, without an extended chromosphere, may be quite possible. But for some typical Be stars, one has the additional evidence of the absorption shells; one also knows that these stars have rotational velocities which are near the stability limit, so that he might expect them to be ejecting matter; one has also evidence indicating that the matter producing the emission lines is concentrated towards the equatorial plane, etc. I think all this adds up to a pretty strong case in favor of a really extended atmosphere rather than the kind of model that you have spoken of.

Now, with regard to Olin Wilson's observations. Some of you may have noticed this morning, when I was concentrating my attention upon the profile of the circumstellar absorption lines in the M giants, that characteristically we get a profile like this at the *K*-line. We have the broad damping wings that are produced in the reversing layer of the star, and in addition we have a chromospheric emission line which is centrally reversed. OLIN WILSON has measured the width of this emission feature, and he finds the extremely remarkable result that it is proportional to the visual luminosity of the star to the one-sixth power, independent of the spectral type, over a range of 15 magnitudes—that is, over a million-fold range in visual luminosity. Question: Does this indicate some kind of turbulent velocity fields in stellar chromospheres, fields which are correlated closely with the visual luminosity? If so, what are these velocity fields like? It is necessary to suppose that the width of the line is the same as the width of the absorption coefficient; or may it be appreciably smaller? One does not know the answers at the present time. However, the existence of emission features of this kind in all late type giants, including the M's, indicates to us the existence of chromospheres for these stars. In these stars, therefore, as in the sun, the temperature falls as we proceed outwards through the photosphere, where the continuum is produced; and then the temperature never gets up to the level of a million-degree corona; or it does get there, passes its maximum, and quickly starts down again so that in our observations we see mainly the cool outer envelope.

— M. J. SEATON:

What is the excitation temperature?

— A. J. DEUTSCH:

Very low; in the outer parts of the envelope, indistinguishable from zero; also the kinetic temperature must be relatively low in the envelope. I would

not like to rule out the possibility that the temperature rises somewhere to heights comparable with what we observe in the solar corona. The parts that we observe, however, are the cooler parts. Finally, I only want to mention that in the  $K$ -line of normal giants the self-reversal normally gets shallower with advancing spectral type, until at  $M_0$  it just about disappears. At about that point there appears a very sharp, very deep absorption feature, which represents the onset of the circumstellar absorption spectrum. As we go to still later spectral types, the violet edge of that feature appears to stay fixed, and the red edge moves longwards. In the more luminous M giants this feature becomes very strong; in the most luminous M supergiants, it may actually absorb away the whole emission line, and we see only a strong deep absorption core at the bottom of the profile of the reversing-layer  $K$ -line.

— R. B. LEIGHTON:

I would like to offer just a very brief comment about Thomas' idea of the structure of the  $K$ -line. One gathers from most of the things we have heard that the chromosphere in which the emission part of that line is formed (and the  $K_3$  absorption) is a uniform thin shell over the outer parts of the surface of the sun and perhaps other stars. Actually we have growing evidence that the emission in the  $K$ -line on the sun comes from extremely well-defined patches distributed quite irregularly over the surface of the sun, and I for one would question very strongly the advisability of trying to interpret a line-profile, which was obtained by some kind of an averaging process over a large area of the sun, in terms of a «source-function». I do not really see how such a «source-function» can be connected with the very spotty nature of the emission in actuality.

— R. N. THOMAS:

The «source-function» is not something you can insert or ignore as you choose—it is the ratio of emissivity to absorptivity and enters in *any* kind of discussion you make which involves transfer of radiation. Our point is simply that if you take a spherically-symmetric chromosphere—but please note the chromosphere is *not* optically thin in these lines—you predict the observed kind of self-reversed emission core. I agree completely that very probably the real chromosphere has departure from spherical-symmetry, and velocity fields, and a complete theory must include these. But note that careful observation of the variation in  $\text{Ca}^+$  emission over the solar surface (*e.g.* E.v.p. SMITH, *Ap. J.*, **132**, 202 (1960)) shows only a change in detailed structure of the self-reversed emission core, no «absence» of these features. Second, how sure is DEUTSCH that there is really a temperature drop in the outer layers of the M giants—the self-reversed core cannot be interpreted as evidence for this, as has been sometimes done.

— A. J. DEUTSCH:

The temperature drop to which I referred is demanded by the observations of the circumstellar lines which appear in the M giants. I am unable to say whether there must be a temperature drop in chromospheres of K giants. I am perfectly prepared to admit that you can reproduce their self-reversed *K*-lines without a temperature drop. But in the M giants it is certainly there.

— S. S. HUANG:

I would like to answer Thomas' original question about whether we can use one and the same mechanism to explain the profile of hydrogen lines observed in Be stars and the similar profile of Ca II, *H* and *K* observed in the sun and in late-type stars. It appears to me that we must invoke two different mechanisms, because in the case of Be stars: 1) the separation of the two emission components is generally of the order of a few hundred km per s, which is too large to be regarded as arising from radiative transfer; 2) their spectra also show the broadened lines arising in the reversing layer of the star and characteristic of rapid rotation, indicating that the deep core of the hydrogen lines is due to a detached ring, and 3) as Miss UNDERHILL and DEUTSCH already pointed out, narrow shell lines which can only be produced in a low-pressure region appear also in their spectra, indicating again the existence of a detached ring. These observed facts led STRUVE to propose that the Be stars are rapidly rotating stars. Their rotational velocities are so large that the equatorial region becomes unstable and mass is ejected from the region. The ejected mass forms a rotating ring in the equatorial plane around the star. Such a ring suffices to produce the profile of the hydrogen lines. Thus, the profile of the hydrogen lines in Be stars is due purely to a geometric effect. This is not so in the case of Ca II, *H* and *K* lines or the Lyman  $\alpha$  line found in the sun, because everywhere on the solar disk we find similar profiles, indicating that the profile can only be explained by radiative transfer. Thus, using the terminology which I presented a few days ago, the profiles of hydrogen lines in Be stars are a result of geometric broadening while the profile of the Lyman  $\alpha$  line found in the sun during rocket flight and perhaps that of the *H* and *K* lines in the late-type stars are the result of physical broadening.

— C. DE JAGER:

The problem of mass-loss of stars in connection with evolution was raised for the first time some 15 years ago by FESSENKOV and Mrs. MASSEVICH in the Soviet Union. At that time they ran into a heavy discussion with the group of HOYLE, MCCREA, LYTTLETON, GOLD, on the question of whether mass-loss or accretion of mass would be the most important for the evolution, and I have a feeling that this battle ended in a draw, resulting in nobody be-

lieving either in accretion or in mass-loss. The problem of mass-loss in connection with the evolution came up again only a few years ago, and this time it came to more general attention of astronomers. DEUTSCH was the first to give a systematical review of data on stellar mass-loss, and I think we all should be thankful to him for that. It is, of course, a first review and we hope that further investigations will soon increase both the accuracy and the number of the data given here.

We turn now to a discussion of the solar observations. There is various evidence for solar mass-loss. First, let me call attention to a rather indirect one, that might otherwise not be mentioned in this symposium. The occultations of the *Crab nebula* by the solar corona take place every year in the month of June. The different observations made since 1950 have shown that these can only be explained if the coronal elements, scattering the radio radiation of the *Crab nebulae*, have elongated forms. The most recent observations by HEWISH, published quite recently show that these elements have shapes as shown in the drawing (furnished by courtesy of A. HEWISH, Cambridge, England):

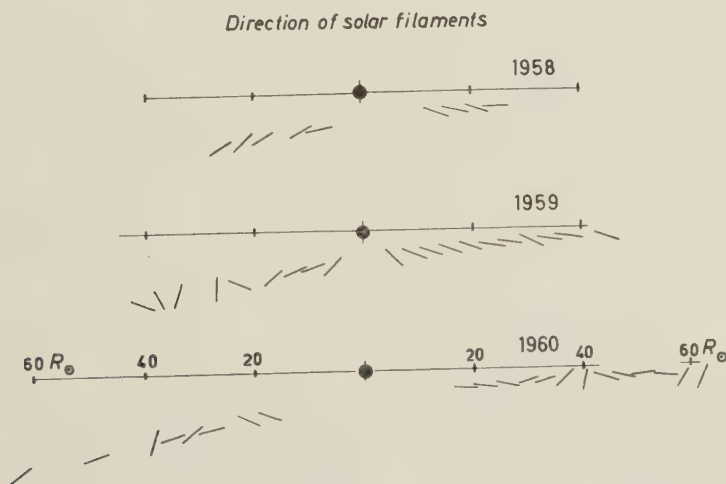


Fig. 1.

It is mostly assumed that matter flying out from the sun follows more or less the magnetic field lines. If this is true the field lines do not indicate a dipole field at great distances from the sun, although the measurements of the polar plumes, near to the sun, suggest a field close to that of a dipole. So we see here an example of matter streaming out and taking the magnetic field with it. I only wanted to mention this observation; I think nothing has been done theoretically on the interaction of outstreaming matter and the field, so it is hardly worth-while to have a discussion on this problem.



— E. LÜST:

I will summarize the work by BIERMANN on comet tails just to give some numbers to indicate what has been found thereby on the mass-loss from the sun. Now, according to BIERMANN, the comet tails of type one, which are of gas and are ionized, can be explained by the interaction of the corpuscular stream coming out from the sun and hitting the comet. The main process which is responsible for the ionization is charge exchange between the protons in the corpuscular stream and the CO molecules in the comet tails; from the observed data on the comet tails, one can get some estimate for the necessary corpuscular flux. The most recent number for the flow is somewhat lower than the values given two years ago. This revision is mainly due to the fact that one has now better experimental observations for the charge exchange cross-section. The cross-section in this energy range is somewhat higher than expected—on the order of  $3 \cdot 10^{-15} \text{ cm}^2$ . Using this cross-section, the corpuscular stream at one astronomic unit is found to be of the order of  $10^9 \text{ ions/cm}^2 \cdot \text{s}$  at the lower level of solar activity. If one assumes an average velocity of about  $500 \text{ km/s}$ —this would lead to a density somewhat less than about  $10^2 \text{ ions/cm}^3$ . This is a rough average value, and the flux might be quite higher on days of higher activity. For instance, the value given by UNSÖLD and CHAPMAN in 1949 for the case of high activity was about  $10^{13} \text{ ions/cm}^2 \cdot \text{s}$ .

The above value of some  $10^9 \text{ ions/cm}^2 \cdot \text{s}$  gives a particle flux at the solar surface of  $10^{11} \text{ ions/cm}^2 \cdot \text{s}$ . Furthermore, assuming a velocity of about  $10 \text{ km}$  per second at the solar surface, this would lead to a density of  $10^8/\text{cm}^3$  involved in the outflow. Also note that the figure on particle flux leads to a yearly solar mass-loss of about  $10^{-13} M_{\odot}$ .

— A. B. SEVERNY:

Blackwell's well-known recent investigations of the corona at great distances from the sun lead to a conclusion that the density of the interplanetary plasma can hardly exceed  $10^2$  particles per  $\text{cm}^3$  at distances  $\sim 1 \text{ a.u.}$  If it were greater, we would be able to find appreciable widening of spectral lines in the spectrum of zodiacal light.

It is also interesting to note the recent Pariysky attempt (USSR Solar Commission session, June 1960) to evaluate the density of solar corpuscular streams by considering the counter glow as formed by these streams. He found that a particle density  $\sim 10^3 \text{ cm}^{-3}$  is sufficient to explain the observed brightness of counter glow.

In connection with this problem of the density of interplanetary space I would like to mention the recent results of measurements of this density with the aid of three Luniks, carried out by GRINGAUS and considered by SHKLOVSKY (K. GRINGAUS, V. KURT, V. MOROS, S. SHKLOVSKY: *Astron. Journal USSR*, No. 4, 1960).

The electrostatic capture devices permitted one to record the current produced by charged particles at energies  $> 200$  V. Near the earth, the density of positive ions falls from  $10^3 \text{ cm}^{-3}$  at the distances 2000 km to  $\sim 10^2 \text{ cm}^{-3}$  at distances  $(28 \div 30)$  thousand km and this run, probably, corresponds to the ionized component of the geo-corona. For interplanetary space between earth and moon, there are no indications of a stationary plasma with particle density exceeding  $(50 \div 80) \text{ cm}^{-3}$  (the usual noise level corresponds to  $\sim 50 \text{ cm}^{-3}$ ). Sometimes comparatively strong currents were recorded in interplanetary space corresponding to a particle flux  $2 \cdot 10^8 \text{ cm}^{-2} \text{ s}^{-1}$ .

Therefore, we think that there is at the present time strong evidence that the interplanetary particle density  $n < 10^2 \text{ cm}^{-3}$  at distances  $\sim 1$  a.u.

— C. DE JAGER:

Let me summarize. We distinguish between the quiet sun, the sun with an activity center, and the sun with a flare. Assume that all the particles that are found at 1 a.u. are moving out from the sun; then we see that BIERMANN, SEVERNY, and BLACKWELL find consistently values smaller than  $10^2$ . Further UNSÖLD and CHAPMAN found that a strong flare emits some  $10^5$  particles  $\text{cm}^{-3}$  at earth's distance. Values for the sun with an activity center are more uncertain, so we put  $10^3$  to  $10^4$ . From the foregoing discussion, it is already clear that flares may give rise to significant increase in the loss of matter, and this need not only apply to the sun but also to the stars.

— J.-C. PECKER:

When one looks at the table given by DEUTSCH, he can see two types of flow: catastrophic and regular. It seems to me that in the solar case, what appears as a regular flow is indeed more or less an integration over time of a lot of processes which are «locally» catastrophic (such as flares). This could be well the case for many «regular» flows. But, in some cases, one sees directly (without integration) the whole catastrophic process. Then I want to ask what happens in the case of the «flare» stars, *UV Ceti*, *T Tauri*, etc. They are similar to the sun in one way because of the surface activity not mentioned by DEUTSCH, but the amount of energy that is involved is much bigger.

— M. MINNAERT:

I should only like to add that the number of M dwarfs is so considerable that the contribution of these stars—if a great proportion of them emits flares—could be perhaps one of the most important contributions of stars to producing interstellar matter. This would mean that instead of considering only the giants there ought to be also a category of dwarfs—the more impor-

tant since we have not yet found processes enough to contribute the necessary amount of matter to the interstellar gas. It might be that Mr. or Mrs. BURBIDGE could give an estimate about the contribution of the M dwarfs.

— G. BURBIDGE:

I do not think that we have the slightest idea about how much matter is ejected by these types of stars. We do know, however, that they do increase in luminosity to a considerable extent, and so we have speculated on the possibility that in the flares which presumably occur at these times, some form of nuclear activity goes on. If it does occur, we shall get large fluxes of comparatively high-energy particles—mainly protons, and these will escape. However, as far as I am aware, one has no idea about the rate at which this will occur. To say that the corpuscular radiation increases in some way proportional to luminosity would, I think, be quite false. There is no observational evidence as far as I know for outward flowing velocity—for expansion velocities in flare stars. So although, as MINNAERT says, this may be an important mechanism of mass-loss from stars, I should point out that these stars are probably in a state of contraction when this process occurs. Consequently this is not quite where we would like it to occur in terms of stellar evolution.

— A. J. DEUTSCH:

Just to reinforce Burbidge's last point. While on the scale of the mass balance of the interstellar medium the contributions of the M dwarfs may be not unimportant, I think that they cannot resolve the difficulty to which I referred, when I pointed out that we are faced with the requirement of getting rid of large quantities of mass from the massive stars.

— N. MILFORD:

Is it important for this conference that we know the mass-loss from stars, since this depends on gravitational parameters which are rather independent of the velocity fields? Secondly, is it very important from the point of view of aerodynamicists whether the flow is  $10^2$  or  $10^4$  or  $10^6$  cm<sup>2</sup>/s in any of these cases?

— E. N. PARKER:

Yes, it is important whether the density is  $10^2$  or  $10^3$ /cm<sup>3</sup> at the orbit of the earth. One can obtain  $10^2$ /cm<sup>2</sup> from the hydrodynamic equations of steady winds with the observed coronal temperature, but he can't by any stretch of the imagination justify  $10^3$ /cm<sup>3</sup> at the orbit of the earth under steady conditions without assuming coronal densities at the sun about 10 times higher than observed.

— N. MILFORD:

This seems to me an astrophysical answer.

— E. N. PARKER:

It seems to me an aerodynamic answer. If one solves the hydrodynamic equations, then he finds one possible range of density, but with definite limitations.

— K. H. BÖHM:

With regard to the question by MINNAERT one should add that according to the observations by HERBIG, there is an apparent outflow of gas from *T Tauri* stars in so far as emission lines in *T Tauri* stars are shifted by about 50 km/s with regard to the absorption lines. Of course, one cannot say whether this is larger than the escape velocity, because one does not know in which region and how far from the star the emission lines are formed; but I think there is some sort of observational evidence in this respect. It should perhaps be noted that HOYLE has claimed that matter is falling in, but this has always been a controversy between theoreticians and observers and so far as I know from HERBIG there has been no evidence of in-falling matter.

— A. UNSÖLD:

Just at the transition point going from observation to theory one more remark about the sun. In the solar corona matter seems to flow out chiefly in the so-called coronal rays. Now in recent times it has become more and more evident that at distances of several solar radii from the sun, matter is quite strongly concentrated in these rays. If you would draw a sphere of say 5 solar radii around the sun, only a small percent of the surface of this sphere would actually be pierced by coronal rays. That means that in considering any problems of flow of matter, or of heat conduction in the corona, we must be aware that the problem is far from spherically symmetrical; instead of the usual factor  $1/R^2$  in the spherical problem we should have a factor more like  $1/R$ . That means to the theoretician that instead of a spherical problem we are dealing with something like the cylindrical problem and that of course may effect the solution quite considerably.

— E. N. PARKER:

You are right that when you look at the solar corona you do in fact see streamers. However, from hydrodynamics it is not obvious that a few solar radii from the sun the material actually flows in the same direction as the streamers. If you work out the equations you find that there is very little difference in the final velocity that you predict, whether you see streamers from the sun or whether you do not.



— R. LÜST:

From comets one has also an indication of some corpuscular streaming at higher latitudes. You see comets at high latitudes and one sees also some activity in these comets. If the theory of corpuscular stream and comet tails of type one is right, you would expect this indeed. Of course, the density drops somewhat if one goes away from the ecliptic, but I do not know the exact factor.

— L. DAVIS:

It is widely accepted that the density of the solar wind is on the order of 100 particles per  $\text{cm}^3$ , and that the velocity is of the order of 500 to 1000 km/s. Any reasonable theory of what happens when this solar wind interacts with the earth's geomagnetic field would indicate that the disturbances go down much closer to the earth than one would think, from the satellite observations made by SENETT and his collaborators at Space Technology Laboratory of the geomagnetic field. There seems to be two possible ways out: One, to say that the solar wind is considerably weaker than all other data indicate—the other is to say that there is something of an aerodynamic nature going on which allows the earth's magnetic field to stick out much further than it has any business to (I do not think this last aspect is something completely understood as yet.) I do not think that you can say that the satellite observations of the magnetic field can be at fault in this point.

— A. UNSÖLD:

As far as the observations go, one can just say that out to 20 solar radii certainly the concentration of matter in particular places is very high. The occultations of the *Orab Nebula*, which DEJAGER mentioned, already are most easily interpreted if one assumes that out there the local density is something like 10 times the average density for the same distance.

— A. J. DEUTSCH:

It is my understanding that CHAMBERLAIN has objected to the very high velocities that DAVIS has said now find general acceptance. Can somebody explain the situation; are there indeed observational reasons for supposing that the velocity will be of the order of 20 km/s.

— E. N. PARKER:

No observational reasons.

— M. KROOK:

I would point out that the radii put in the various tables may not have any direct significance for the structure or the dynamics of the stellar atmosphere. These radii are wholly based on observations and theories de-

signed to explain observations in a very narrow spectral range—the visual range—and the stellar radius is established from observations in this visual range. It is instructive to consider what would have happened had we been able to observe only in the radio-frequency range; we would perceive only the outer parts of the corona. Would we have been able to produce a model of the sun? Where the star ends, and where the interstellar medium begins, is a vague idea; and one must be careful in discussing radii and distances.

— A. J. DEUTSCH:

I would object. Certainly the «edge» of the star is not well-defined; but it would be unfair to imply that it has no physical significance. It is the place where the mean-free-path of the average photon suddenly increases enough to permit escape into space. It is no accident that we observe stars not many octaves from the region where the continuous spectrum reaches its peak intensity.

— M. KROOK:

I think you are implying that the structure of the outer layers of the star is completely determined by what you see in the optical region. Once this is clear, everything else is. The idea is that there is no reaction back on the atmospheric structure by what lies outside its edge.

— E. N. PARKER:

I would like to pursue the proposal made two years ago that the solar corona is in a state of continual hydrodynamic expansion. Let me start by commenting on the question raised by UNSÖLD, the extent to which one can apply spherical symmetric calculations. We have investigated this problem because it is obvious, looking at eclipse photographs of the sun, that the sun is *not* spherically symmetric. We find that whether the gas moves out along a radius or along some sort of a flat fan, we do not obtain even a 15 percent difference in the velocity for a given input of coronal heating. The velocities are remarkably insensitive to the kind of geometry, since after all the stationary flow equations are nothing more than conservation of energy. So I will go ahead with the spherical case. Let me cite some of the evidence for an out-flow of gas through interplanetary space—LÜST fortunately has gone over most of the observations already so that I do not need to treat them at length. I want to distinguish the quiet sun from what I call the active sun, *i.e.*, the sun immediately following a large flare. The best evidence by far, I think, is from Biermann's comet analysis, which you remember gives densities on the order of 100 particles per  $\text{cm}^3$  at the orbit of earth and flow velocities on the order of a few hundred km/s, for the quiet sun. This is apparently a perpetual state for the sun, not only in the plane of the ecliptic, but far from

the plane of the ecliptic. There is other evidence that there is continuous corpuscular radiation from the sun, and that is the quiet day aurora. Every clear night in the auroral zone one sees the aurora. If we believe that the aurora is due to corpuscles from the sun, it would imply that every day there is corpuscular radiation from the sun. In the same way one observes continual polar magnetic agitation, which presumably is also due to corpuscular radiation from the sun. Since the agitation is a continued state in the polar regions, one again comes to the conclusion that there must be continual corpuscular radiation from the sun. There are a number of other arguments that one can give here, but I think these are typical, and perhaps the best of the lot. In contrast to the quiet sun, we have the active sun, for which the estimates of particle density due to agitation at the orbit of the earth run as high as  $10^5$ . The one or two day transit times between the observed flare and arrival of something at the earth, give velocities somewhere between one and two thousand km/s, so let me write down 1500 km/s as a typical figure. The density of  $10^5/\text{cm}^3$  and the velocity 1500 km/s are entirely consistent with the low latitude aurora and with the magnetic storms which one sees to follow the flare.

Now the question is, what is the origin of this solar corpuscular radiation. I want to pursue the suggestion made two years ago that the « solar corpuscular radiation » is nothing more than hydrodynamic expansion of the solar corona. The solar corona is very hot, and it is simply a matter of solving the hydrodynamic equations to see if the corona is hot enough to expand with the velocities and densities just given. We ask under what circumstances the corona of a star such as the sun will expand, and under what other circumstances might it be static. Suppose that I can observe the temperature of the corona of a star out to some distance  $r$ , and beyond that I cannot observe them. I must therefore speculate as to what happens beyond  $r$ . Let me take the best case that I can for a static corona. Suppose that the temperature out to  $r$  is  $T(r) = 2 \cdot 10^6$  °K. How might I best maintain this corona in static equilibrium? You begin exactly at the limit of observation and put an adiabatic atmosphere on top. You cannot have the temperature drop more rapidly than the adiabatic gradient beyond  $r$ , because you would then get convective overturning—but you can postulate that it drops adiabatically. A static atmosphere requires that  $\lambda = GM_s M / r k T(r) \geq \frac{5}{2}$ , where  $M_s$  is the mass of the star,  $M$  the mean mass of an atom in the corona, and  $G$  the gravitational constant, *i.e.*, the gravitational energy must be larger than some fraction of the thermal energy, or it cannot hold an adiabatic atmosphere. Unless the inequality is satisfied, the pressure does not fall to zero at infinity; you would have to enclose the star in a box to maintain its atmosphere static. So look at the sun and ask what the criterion tells us there. POTTASCH and CHAPMAN have recently independently investigated the temperature of the corona at sunspot

minimum using Blackwell's density observations out to about 22 solar radii. From the density observations, one can compute the gradient of the density. The density gradient and temperature can be related if we are talking about a static corona, so we assume that the corona is static, as did POTTASCH and CHAPMAN. This will give us a lower limit on the temperature. One finds that the number  $\lambda$  in close to the sun is very large. The gravitational forces are large enough to satisfy the inequality. But  $\lambda$  decreases outward from the sun. We find that  $\lambda$  reaches  $\frac{5}{2}$  at the  $22 R_{\odot}$  limit of observations. It is hard to make a definite statement of accuracy here; for BLACKWELL gives no definite statement on the accuracy of his observations in this region. POTTASCH and CHAPMAN differ by about 7 percent, so I use that as some kind of estimate of error, which makes the value of  $\lambda$  very close to  $\frac{5}{2}$ . So either the corona must become exactly adiabatic, or I cannot have a static corona.

— S. POTTASCH:

Beginning at about 10 solar radii, Blackwell's values of density depend somewhat on unavailable knowledge of the brightness much further out. So the densities are uncertain by about a factor 2.

— E. N. PARKER:

Since the density is changing by factors of ten, you do not need much accuracy to tell the difference between, *e.g.* the isothermal and adiabatic cases. So it seems to me necessary to abandon a static corona. Then, we turn to investigate the possibility of expansion.

In the same notation, the hydrodynamic equations of motion are, for spherical symmetry:

$$(1) \quad \text{dynamic:} \quad NM \, dv/dr = -d(2NkT)/dr - GM_{\odot}NM/r^2$$

$$(2) \quad \text{continuity:} \quad Nvr^2 = N_0v_0r_0^2$$

we assume that  $T$  and  $N$  are related by the polytropic law:

$$(3) \quad T = T_0(N/N_0)^{\alpha-1}.$$

We can integrate the equations completely for a general value of  $\alpha$ . Let  $\psi$  be the kinetic energy of the average ion in units of the initial thermal energy at the base of the corona, which I take to be  $r = a$ . Let  $\lambda$  be the gravitational parameter as defined above. This is essentially the gravitational potential energy per atom at the base of the corona in units of the thermal energy at that same point. Let  $\xi = r/a$ . The solutions can then be written in this



form:

$$(4) \quad \psi \equiv \frac{Mv^2}{2kT_0},$$

$$(5) \quad \psi = \psi_0 - \lambda \left(1 - \frac{1}{\xi}\right) + \frac{2\alpha}{\alpha - 1} \left\{ 1 - \left( \frac{\psi_0}{\psi \xi} \right)^{(\alpha-1)/2} \right\},$$

$$(6) \quad \psi - \ln \psi = \psi_0 - \ln \psi_0 + 4 \ln \xi - \lambda(1 - 1/\xi).$$

We denote by  $\psi_0$  the value of  $\psi$  at  $r = a$ . We arbitrarily choose  $a = 16$  km to be the base of the corona at which we specify the density  $N_0$ , temperature  $T_0$ , and velocity  $v_0$ . We ask what solutions are appropriate to the sun. It is not interesting to start above the speed of sound at  $r = a$  because that begs the question. Let me start with some exceedingly low velocity, 1 km/s. I find

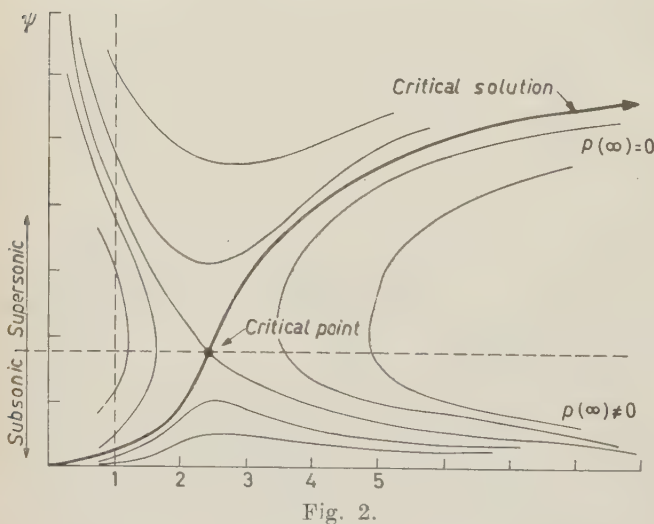


Fig. 2.

that my solution has the property: it rises and then soon falls again. I find that in fact it falls so rapidly that the density and pressure do not go to zero at infinity, where we have only the very small interstellar gas pressure of perhaps  $10^{-14}$  dyne/cm<sup>2</sup>. In fact the pressure at infinity for this solution is a few percent of that at the base of the corona—enormous!

Consider such a solution for which  $\psi_0 \ll 1$ , so that the pressure does not go to zero at infinity. Obviously I need a box, enclosing the sun at infinity, to maintain the pressure and the stationary character of the flow. Suppose that I slowly dissolve the box. With the decreasing back pressure at infinity, the flow will accelerate.  $\psi_0$  will slowly increase. This does not cause the pressure at infinity to go to zero until finally I come to the critical solution, shown in the figure, which starts at relatively low velocity on the sun, with  $\psi_0$  corresponding to 50 km/s at  $r = a$ . At this point the solution changes its asymptotic form at large  $\xi$ . The velocity ceases to go to zero, and the density and pressure suddenly do go to zero. There are no physically meaningful solutions for  $\psi_0$  greater than the critical value. Thus, we have the critical so-

lution, which you see breaks away from the solutions on both sides of it and goes up to a finite velocity at infinity. The only exception to this arises when you insist upon an isothermal corona all the way to infinity, in which case it goes up logarithmically. For all other cases it levels off, giving you constant velocity, and therefore a density which goes to zero like  $1/r^2$ , with zero pressure at infinity. This is, in fact, the required boundary condition. Such are the characteristics of these general solutions. We have plotted them for  $1 \leq \alpha \leq \frac{5}{3}$  and for many values of  $\lambda$ . One can compute the many asymptotic relations, but that is not terribly interesting for a qualitative discussion such as this. The point is that if we believe the pressure to vanish at infinity, *i.e.* no box enclosing the sun, then the corona must follow this critical solution. The velocity at the base of the corona is of the order of tens of km/s.

(Note that « base of the corona » refers to a radial distance  $3 \cdot 10^5$  km above the photosphere— $10^6$  km from the center of the sun—where the particle density is about  $3 \cdot 10^7/\text{cm}^3$ , probably a bit lower at sunspot minimum, and a bit higher at maximum.)

— P. LEDOUX:

What is the total mass of the expanding atmosphere?

— E. N. PARKER:

The same as the existing corona, because the mass of the corona is essentially contained in the first scale-height.

— P. LEDOUX:

But you have a density decreasing as  $R^{-2}$  which, integrated to infinity, gives an infinite mass. If you assume that you consider a transient stage, you will have some kind of front at the most external point reached, and boundary conditions there should fix the motion—you will not need to go to infinity. But the solution you have discussed is selected on the basis of a boundary condition at infinity—not to have a finite pressure there.

— E. N. PARKER:

The point on the integration would be true if you really took the solution extending to infinity, but sooner or later the interstellar medium must stop the flow. And, on specifying the pressure, all I need to do is replace zero by  $10^{-14}$  dyne·cm $^{-2}$ , the pressure of the interstellar medium, and I will reach the same conclusion on the results. The critical aspect of the solution is to end up with a very low pressure, rather than with a factor only 10 or  $10^2$  less than that at the base of the corona, which is some  $10^{-2}$  dyne·cm $^{-2}$  corresponding to the values assumed.

— F. KAHN:

Am I right in thinking that these equations are the same as Bondi's equations for the case of spherically symmetrical accretion?

— E. N. PARKER:

I am sure that they must be. Only the sign of the velocity would be changed.

— F. KAHN:

In that case, would the stable solution be the one which is subsonic at  $\infty$  and supersonic near the star?

— E. N. PARKER:

He would probably have used an effective  $\alpha \leq \frac{5}{3}$  with small velocity and pressure at infinity. It is hard to compare two such unlike situations.

— F. KAHN:

There must be some reason why one flow is stable for outward motions and unstable for inward motion.

— A. J. DEUTSCH:

I believe that this solution is probably the one appropriate to describe the solar corona, but I cannot agree with the remark that one recovers the same result if he requires either the solution which goes to  $10^{-14}$  dyne/cm<sup>2</sup> at infinity, or that which goes to zero. One can obtain adiabatic solutions which go exactly to  $10^{-14}$  dyne/cm<sup>2</sup>, or to any other prescribed value, and they get there with zero velocity. Parker's solutions do not have that character. If this is of any physical relevance (and I am not persuaded that it is or is not), it may well be quite an important distinction. The next point, which is related to this: it is not immediately clear to me why, if one is uncomfortable with a solution which yields a pressure many orders of magnitude too high at infinity to be balanced by the interstellar medium—and incidentally a non-zero velocity—he is nevertheless prepared to accept a solution which yields a pressure many orders of magnitude too low, and a finite velocity.

— F. KAHN:

I wonder whether there is any use in having a solution which is subsonic at infinity. You have an interstellar medium moving relatively to the stars at a speed that is much higher than the sonic velocity at infinity. The usual speeds are to 10 to 20 km/s, sonic speeds are of the order of 1 km/s. So you could not possibly fit a subsonic solution at all.

— W. M. MCCREA:

These are indeed exactly the same questions as for symmetrical inflow which were solved first by BONDI, and about the same time by EBERT, and they obtained the complete set of solutions as Parker has drawn it. Now for inflow—as I showed afterwards, the likely thing is for the motion to follow one solution curve a certain distance and then to get a shock and then to follow a different solution curve. I do not know whether the aerodynamicists agree but I think that this is what happens. Now I spent a lot of time, myself and a pupil, trying to get outflow, and I could not get anything plausible. I got this solution that Parker has talked about. But you have got to give the material the velocity at some level. And that is the whole problem—you can always get the solution if you get the exact initial condition, but we could not see how this could come about in reality. The only hope that I could see is if you have a solution that is essentially non-steady, and carry the solution right down to the center of the star, and have a star which instead of being in static equilibrium is somehow an expanding system, where the velocity of expansion deep down is small but not zero and is appreciable in the region of interest. But that was too difficult a problem for me. I think that aerodynamicists ought to say what they think.

— A. J. DEUTSCH:

I would keep open the possibility that the conditions of some appropriate fit onto the interstellar medium may indeed produce a disturbance that will propagate into a distance of not many stellar radii. I would agree that the fitting requirements on the interstellar medium may make no difference at the surface of the star, but I am not prepared to admit that the corona even as close as 2-3 radii is equally insensitive. My principal reason for this insistence is that if we admit the relevance of these boundary conditions, we can reproduce within an order of magnitude *both* the temperature and density of the solar corona. This impresses me as being too much for chance coincidence.

— E. N. PARKER:

If you feel that there is something to this point, it should be calculated. I think you are right that the temperature and density are due to the interstellar medium rather than to the convective zone of the sun.

— L. DAVIS:

If one is concerned about boundary conditions between expanding medium and interstellar space, he should include the pressure of the interstellar magnetic fields, which are probably 100 times the gas pressure and are anisotropic. So long as you are not worried about these boundary conditions, you do not have to worry about the interstellar magnetic field.



— L. BIERMANN:

I would suggest this magnetic pressure might be as large as  $10^{-11}$  dyne/cm<sup>2</sup>.

— E. N. PARKER:

What would happen is that the solar wind would rush outward until its momentum density equals whatever pressures there are, and at that point you get some sort of disordered interface.

— C. DE JAGER:

In the case of the sun, we are dealing with rather special boundary conditions because the sun is surrounded by the planetary system, and I have the feeling that in the region of the planetoids there is an additional source of turbulent magnetic fields. The system of the planetoids surrounding the sun is known to consist of a number of large planetoids, a greater number of smaller particles, an enormous number of little stones and dust and so on, and the many collisions between these particles may produce, by evaporation, turbulent gas, that will be ionized by the sun. The turbulent masses of gas could produce turbulent magnetic fields. Evidence for these turbulent magnetic fields is found by cosmic ray observations. As SIMPSON showed, these observations of scattering of solar cosmic rays may be explained by assuming that the interplanetary magnetic fields are greatly turbulent, say, at distances beyond the distance of the earth. So, I have the feeling that the magnetic pressure at this distance may be considerably higher than the magnetic pressure in the interstellar medium. So, if we want to discuss the problem of the outflow of matter from the sun, we must take into account that already at distances of about one or two a.u. it collides against this turbulent field, which will act as a filter to the outgoing matter, which still will go out finally, but much more slowly.

— A. B. SEVERNY:

I would appreciate a comment on the applicability of a hydrodynamical treatment of the problem, because the mean-free-path may be something like the sun-earth distance.

— E. N. PARKER:

The mean-free-path is comparatively short—about 1/10-th the characteristic dynamical length.

— E. SCHATZMAN:

You may have at least two kinds of discontinuity: You may have an ordinary shock and you also can have a change from an H II to an H I region

and then in the  $\xi, \psi$  plane I think that you shift from one point on one of your curves to some other point. I think that at least in some cases that is part of the difficulty.

— E. N. PARKER:

Let me put some dimensions on here. The critical point occurs within 2 or 3 solar radii of the sun, and once you are well past the critical point you can completely neglect the internal state of gas—it is on its way at 500 km/s. It is true that if I were trying to compute the temperature of the gas at the orbit of the earth I would have to pay very close attention to these things. But as a matter of fact the velocity and density of the mass flow depend upon them hardly at all. The expanding gas has climbed out of a 600 km/s potential well, which is equivalent to about one kilovolt per hydrogen atom.

Let me make one comment about McCrea's question as to what I do about the initial velocity of the order of 20 or 30 km/s in the corona. The initial velocity at the arbitrary radius  $r=a$  goes to zero if I chose to decrease  $a$ . If I arbitrarily start at one million km from the center of the sun—of course the velocity is not zero.

— W. M. MCCREA:

That is the point I was drawing attention to on the other board—it is not a steady problem essentially. You can't go right to the center. You would have point sources.

— E. N. PARKER:

I would like now to comment upon the extension of the solar magnetic field into interplanetary space. If there is, in fact, hydrodynamic radial expansion of the solar corona, then it is obvious that the expansion must pull out the general solar field into a radial configuration. I don't know whether the general field is a dipole—but whatever it is, any lines leaving the surface of the sun which are not stronger than about one gauss are pulled out into some more or less radial configuration; I am sure that the fields play a role in the formation of the coronal streamers, as UNSÖLD has mentioned, but none the less they are stretched out in what I would call a roughly radial configuration. Because the sun rotates, they spiral slightly. To give an estimate of the spiraling, a 400 km/s wind at the orbit of the earth results in a spiral which just reaches  $45^\circ$ . Hence inside the orbit of the earth the field is principally radial, and outside the orbit of the earth it is principally azimuthal. I make this point because I shall need it when speaking briefly on the active sun.

Some times on the sun in an active region there is an enormous flare, and one observes that the solar corona over a large region above the flare rises

very quickly to sometimes 4 million degrees. As soon as the flare is over you see that the temperature is roughly doubled, and it remains high for a day or two or three—sometimes even a week—thereafter. The point I would like to make is that this suggests that a hydrodynamic explosion must take place. So I have investigated the hydrodynamics of blast waves from the sun. Again I assume spherical symmetry, and this time I agree that it is not a very good approximation. But let me make this one point—if I assume spherical symmetry about the center of the sun, I will get a lower limit on the velocity, for the same temperature profile. So I am not overestimating the velocities with the spherical symmetry approximation. One uses the standard techniques  $\eta = \tau/r^2$ —the similarity variables for progressive waves—it is all in COURANT and FRIEDRICHS, and I will not bother you with it.

The density ahead of these waves falls off like  $1/r^2$ , from the quiet day solar wind model where the velocity is roughly constant at large radial distance. One assumes that the thermal velocities are small compared to the shock velocities, so the Mach number is large—so you get a factor of 4 increase in the density across the shock at the head of the blast wave. Now, if a flare, and the corona over the flare, were merely a single explosion, so that the energy were all added in an hour and no energy added thereafter, then you have a true blast wave—the density would rise by a factor of 4 and fall again behind the shock, with the parameter  $\chi = \frac{3}{2}$ . ROGERS worked out this solution several years ago. Now it is observed that the solar corona remains at its elevated temperature of four million degrees for a day or so thereafter. So there is the possibility that it will expand and continue to push on the back of the blast wave. That is what the cases represent,  $1 < \chi \leq \frac{3}{2}$ . If I take an extreme case where I assume that the corona pushes on the blast wave so hard that the kinetic energy of the blast wave increases linearly with time, then I get the step wave for  $\chi = 1$ , which is quite thin. On the sunward side of the rear of the blast wave there is nothing but hot coronal gas pushing outwards. I think that  $\chi = 1$  is an extreme case, of course. Now if the corona pushes so that the energy goes up like  $t^3$ , one obtains another curve, etc. So I offer you a sequence of blast wave profiles—one will have to decide from the observations which is appropriate.

The question is, do these blast waves have the right velocities and densities to agree with what rough observations we have? At  $4 \cdot 10^6$  °K we go to our stationary solutions for the hot coronal gas driving the blast wave, and ask are there any solutions flowing outward with a thousand or 1500 km/s velocities? The answer is yes, 1200 km per s is a rough estimate of the rate at which the rear of the blast wave might be driven outward by a  $4 \cdot 10^6$  °K corona. The front of the blast wave automatically goes faster, at 1500 km per s or higher. The most extreme figure quoted for the density is  $10^5/\text{cm}^3$  at earth, and all I can say is that the blast wave densities can match that.

If the corona is several million degrees all the way to the back of the blast wave, then the pressures are so high in back of the blast wave that the density can in fact be that high. I do not wish to argue the point one way or another. The blast wave from a  $4 \cdot 10^6$  °K corona can duplicate the 1500 km/s and  $10^5/\text{cm}^3$  suggested from observation. Our hydrodynamic model of both the quiet and the active sun accounts for the observed solar corpuscular radiation.

Now consider the cosmic ray intensity in interplanetary space, which is how I got started on this whole calculation. I am immediately concerned with the magnetic field configuration in interplanetary space. In the quiet-day solar wind, the lines of force of the general one gauss solar field are drawn out in Archimedes spirals, reaching  $45^\circ$  from the radial direction at earth, as I have already mentioned. The blast wave energy, increasing like  $t^2$ , distorts the quiet day field. And now what effect does this have on the cosmic ray intensity? It is very easy to compute the extent to which cosmic rays are swept out of the inner solar system by outward sweeping magnetic fields. You find that you get up to 40 percent cosmic ray decreases with an energy dependence which is something like reciprocal magnetic rigidity, which is in rough agreement with the crude observation that currently exists. Such a decrease is immediately recognizable as the Forbush type cosmic ray decrease, observed following a given flare on the sun, simultaneously with a magnetic storm on earth.

— W. H. MCCREA:

Is this the dipole field that you are distorting?

— E. N. PARKER:

I do not wish to commit myself as to whether the general solar field is a dipole. Any lines of force ( $B \leq \text{one gauss}$ ) will be drawn out as I have shown them, regardless of how the field density varies over the surface of the sun.

— L. DAVIS:

We have seen some figures showing magnetic fields in the solar system and heard some discussion of this. In the last few months Pioneer V has reported back observations on one component of the magnetic field in the solar system over a period of some 50 days, and following around the orbit of the earth, but with the satellite going somewhat towards the orbit of Venus during this time. The magnetic field during this time as reported by SENETT and his collaborators at Space Technology Laboratories is very hard to fit to any ideas that one has. It is just possible that the apparatus is inhabited by a gremlin that is trying to upset it—but there is nothing in the experiment that



would indicate this except that the results are so surprising. Something like a sixth to a quarter of the time the field is surprisingly uniform and steady at about  $2.5 \times 10^{-5}$  gauss. More than half of the time the fields show quite a bit of irregularity. It may run up to  $4 \cdot 10^{-4}$  gauss on some occasions. These disturbances seem to be correlated with geomagnetic disturbances which would indicate that it really was not due to gremlins. In any case they seem to indicate that more than half the time there is a much more irregular magnetic field than Parker's figures would seem to indicate, and the direction seems completely wrong.

— A. UNSÖLD:

A word on the problem of the temperature and density gradients in the corona. The coronal density gradient leads to temperatures of about  $1.6 \cdot 10^6$  while the line-profiles consistently give  $2.5 \cdot 10^6$ . I have wondered at the reason for this discrepancy, and I think it is the ray-structure of the corona. The corona essentially follows systems of lines of force, and VAN DE HULST showed long ago that a tube of magnetic lines of force is filled up from below like an isothermal atmosphere without regard to sideward limitation. If one wants to bring into agreement the stratification measured by BLACKWELL for the average corona, on the one hand, and the higher temperature of  $2.5 \cdot 10^6$  on the other hand, one must make suitable assumptions as to how the coronal rays thin away further away from the sun. One finds *e.g.* that at 5 solar radii, roughly  $\frac{1}{5}$  of the sphere is crossed by such rays. Such a degree of inhomogeneity agrees well with what ALLEN deduced several years ago from the waviness of the observed isophotes. This picture seems to have various implications. For instance, the more or less explosive way described by PARKER of producing transient phenomena is certainly not the only possibility. Radio observers know since a long time that the so-called slowly variable radio frequency radiation in the decimeter range is due to « coronal condensations » which often pass into coronal rays. These are often produced by the following mechanism. The amount of matter in each tube of force is simply proportional to the density at which the coronal region begins, and that critical density is higher if the mechanical flux is higher. So one would explain these rather quiet columns of gas essentially by just assuming that they have the same density gradient as their less dense surroundings, but that the density is everywhere multiplied by a certain factor which observations show to be about 5 or 10. It may of course be that there are other possibilities of getting matter into higher layers.

— E. N. PARKER:

This is an interesting idea. Do you have in mind the corpuscular radiation following a flare, when you discuss this more or less quiet streaming?

— A. UNSÖLD:

No. My opinion is that we have two different possibilities which apply to two different phenomena. Also, our opinions seem to differ somewhat as to the coronal temperature—I put the average value a bit higher. Then, I would emphasize that the curves fitted to Blackwell's data largely reflect how the magnetic tubes of force thin out.

— A. UNDERHILL:

The flow velocities from stars do not seem to exceed a value of  $10^3$  km/s by more than a factor of 2 except in the supernovae. Is there any hydrodynamical reason why we do not observe velocities as high as  $10^4$  km/s? It does not matter what kind of star you take—the outflow velocities apparently lie between 10 and about 1000 km/s.

— E. N. PARKER:

I will attempt an answer. Simply remember that the velocity goes up only as the square-root of the energy; and even if you take 10 or 20 million degrees, it is hard to beat 1000 km/s.

— W. H. MCCREA:

I make one simple observation, which is not meant to be cynical, although it sounds like it. Fifteen years ago, HOYLE, BONDI, and LYTTLETON explained the solar corona by infalling material and got a suitable density gradient. Here, you reverse all the velocities; naturally you get the same density gradient.

## II.

### Re-discussion, from the Wiewpoint of Aerodynamics.

*Chairman:* H. LIEPMANN

### Remarks on Steady Perfect Fluid Flow with Spherical Symmetry.

P. GERMAIN

*Institute Henry Poincaré - Paris*

#### 1. - Introduction.

As discussed by DEUTSCH, the following gas-dynamical problem arises in the study of a stellar atmosphere: find a steady outward flow, originating from a star, which is compatible with the interstellar medium. In what follows, spherical symmetry will be assumed; moreover, the gas will be considered as a perfect gas with constant specific heats.

The notation is as follows:

$r$ , distance from the center of the star;

$\varrho$ , density;

$\tau$ , specific volume;

$T$ , absolute temperature;

$C_p$ , specific heat per unit mass at constant pressure;

$\gamma$ , adiabatic index ( $1 < \gamma \leq \frac{5}{3}$ );

$GM/r$ , gravitational potential of the star;

$\tau_\infty$ ,  $T_\infty$ , values of  $\tau$  and  $T$  in the interstellar medium which are assumed to be known.

#### 2. - Equations.

This flow is ruled by the usual conservation laws: mass, momentum and energy. The continuity equation gives:

$$(1) \quad \varrho u r^2 = m \quad \text{or} \quad u = m \tau r^{-2},$$

$m$ , the rate of mass flux, is a constant.

The energy equation says that the specific entropy is a constant; as a result the momentum equation may be replaced by the Bernoulli theorem

$$(2) \quad \frac{u^2}{2} + C_p T - \frac{GM}{r} = B,$$

while the equation of state shows that

$$(3) \quad T\tau^{\gamma-1} = \text{const}.$$

The physical problem is a study of mass-loss from the star; thus  $B > 0$ . For large values of  $r$ , as  $\tau$  tends towards  $\tau_\infty$ ,  $u$  tends to zero, and (2) shows that  $B = C_p T_\infty$ . Thus,  $B$  is a known positive constant. It is very easy to study the variations of  $u$ ,  $\tau$ ,  $T$  as functions of  $r$ . For instance  $\tau(r)$  is implicitly given by

$$(4) \quad \left( B + \frac{GM}{r} - \frac{m^2 \tau^2}{2r^2} \right) \tau^{\gamma-1} = \text{const}.$$

It is convenient to introduce non-dimensional variables.

If  $r_L$  (length) and  $\tau_L$  (specific volume) are defined by

$$(5) \quad GM = Br_L, \quad m^2 \tau_L^2 = r_L^4 B$$

and if one introduces

$$(6) \quad x = \frac{r}{r_L}, \quad y = \frac{\tau}{\tau_L},$$

(4) may be written

$$(7) \quad F(x, y) = \left( 1 + \frac{1}{x} - \frac{y^2}{2x^4} \right) y^{\gamma-1} = C,$$

where  $C$  is a constant ( $C > 0$ ).

One must investigate the shape of the curves ( $\zeta$ ) defined by (7) in the domain  $x > 0$ ,  $y > 0$ .

### 3. - Pattern of the curves ( $\zeta$ ).

First of all, one has

$$(8) \quad \frac{\partial F}{\partial x} = \frac{y^{\gamma-1}}{x^5} (-x^3 + 2y^2), \quad \frac{\partial F}{\partial y} = \frac{y^{\gamma-2}}{x^4} \left[ (\gamma-1)(x^3 + x^4) - \frac{\gamma-1}{2} y^2 \right].$$



a) The curve  $(\bar{I})$  defined by

$$2y^2 = x^3$$

is the locus of points of  $(\zeta)$  where the tangent is parallel to the  $x$  axis.

For a given  $y$ ,  $C$  is an increasing function of  $x$  for  $x^3 < 2y^2$  and a decreasing function of  $x$  for  $x^3 > 2y^2$ . Every curve  $(\zeta)$  is cut in at most two points by a parallel to the  $x$  axis.

b) The curve  $(I_*)$  defined by

$$(10) \quad y^2 = \frac{2(\gamma - 1)}{\gamma + 1} (x^3 + x^4),$$

is the locus of points of  $(\zeta)$  where the tangent is parallel to the  $y$  axis. As

$$C_x T = \frac{c^2}{\gamma - 1},$$

with  $c$  the speed of sound, it is easily checked with (2) that  $(I'_*)$  is the locus of points which correspond to a Mach number equal to 1. This curve  $(I'_*)$  divides the domain into a subsonic and a supersonic region. That is, for a given  $x$ , a value of  $y$  exceeding that from eq. (10) requires supersonic flow; less, subsonic.

For a given  $x$ ,  $C$  is an increasing function of  $y$  for

$$y^2 < \frac{2(\gamma - 1)}{\gamma + 1} (x^3 + x^4),$$

and a decreasing function of  $y$  for

$$y^2 > \frac{2(\gamma - 1)}{\gamma + 1} (x^3 + x^4).$$

Every curve  $(\zeta)$  is cut in two points by a parallel to the axis.

c) When  $1 < \gamma < \frac{5}{3}$ :

$(\bar{I})$  and  $(I'_*)$  have one point of intersection  $A$ , apart from the origin

$$(11) \quad x_A = \frac{5 - 3\gamma}{4(\gamma - 1)}, \quad y_A^2 = \frac{1}{2^7} \left[ \frac{5 - 3\gamma}{\gamma - 1} \right]^3.$$

Obviously,  $A$  is a saddle point for the family of the  $\zeta$ -curves. The particular



Notice that (10)—the equation of  $(\Gamma_*)$ —appears as a special case of (14).

In the  $x, y$  plane, when a shock is present the image point of the flow jumps from a curve  $(\zeta_1)$  to another  $(\zeta_2)$ . It is important to compare the values of the related constants  $C_1$  and  $C_2$ . If one notes that the constant in the right-hand side of (3) is proportional to

$$\exp \left[ \frac{s}{C_v} \right],$$

where  $s$  is the specific entropy and  $C_v$  the specific heat at constant volume, it is clear that  $C_2/C_1$  is proportional to

$$\exp \left[ \frac{s_2 - s_1}{C_v} \right],$$

and, as  $s_2 > s_1$ , one concludes that

$$(15) \quad C_2 > C_1.$$

## 5. - Discussion of uniqueness.

Let us recall that it is always assumed that  $\tau_\infty$  and  $T_\infty$  are given. Thus the constant  $B = C_v T_\infty$  is known. The following assumptions are also considered

a)  $m$  is given;

b) the radius of the star is very small in comparison with  $r_L$ ; and in its neighborhood, the flow is subsonic. This excludes for the moment the special limiting case  $\gamma = \frac{5}{3}$ .

This last assumption means that in the  $x, y$  plane, the image of the flow is to be defined for  $0 < x < \infty$ , and that the curve must reach the origin in a path lying in the subsonic region in the neighborhood of this point. Of course, it may be a serious limitation, which has to be discussed with figures coming from the observations, but without such an assumption uniqueness cannot be proved.

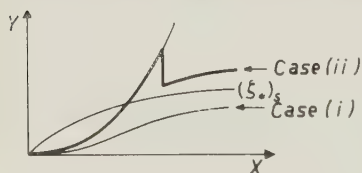


Fig. 4.

With these assumptions, the flow is uniquely defined.

First of all,  $r_L$  and  $\tau_L$  may be computed by (5). Thus  $y_\infty$  is known and defines uniquely the  $(\zeta)$  curve for large  $x$ . Let us call  $(\zeta_\infty)$  this arc. Two cases are possible (Fig. 4).

i) This arc  $(\zeta_\infty)$  lies below the arc  $(\zeta_*)$ , which goes through  $A$ . The corresponding  $(\zeta_\infty)$  curve goes through the origin and checks the assumption  $b$ ). Thus, it is a solution. No other solution is possible because according to (15), such an arc  $(\zeta_\infty)$  would be the image of a flow lying downwards of a shock, if the image of the upward flow were a  $(\zeta)$  curve lying above the two branches of the critical curve  $(\zeta_*)$  and thus condition  $b$ ) would not be satisfied.

(ii) Or this arc  $(\zeta_\infty)$  lies above  $(\zeta_*)_s$ . A shock must be present. For the reason given in i), the only possibility to check assumption  $b$ ) is to have as image of the upward part of the flow an arc of curve  $(\zeta_*)_s$  which corresponds to a flow starting as a subsonic flow for  $x = 0$  and which is accelerated smoothly through the trans-sonic regime (saddle point  $A$ ).

The position of the shock is defined as the intersection of the  $(\zeta_\infty)$  curve with curve  $(\zeta)$ , which represents the locus of flows lying just behind a shock when the above state lies on the  $(\zeta_*)_s$  branch. It may be checked that these two curves have one and only one point of intersection.

The special case  $\gamma = \frac{5}{3}$  is a limiting case whose discussion is left to the reader. We want to emphasize that assumption  $b$ ) is necessary to guarantee the uniqueness property.

## 6. — Signification of the results.

The values of  $\tau_\infty$ ,  $T_\infty$ ,  $B$  are given by the data of the interstellar medium. But the value of  $m$  depends on data on the state observed on the surface of the star. Let us note with a subscript the corresponding values of various quantities ( $r_0$ ,  $\tau_0$ ,  $T_0$ ...)

$$m = u_0 r_0^2 \tau_0^{-1}.$$

In order to compute  $m$ ,  $u_0$  and  $\tau_0$  (or  $T_0$  and  $\tau_0$  according to the Bernoulli equation) must be known. To compare the theory with experimental data, one must check that the flow on the star is subsonic and that the flow at  $r_0$  may be compatible with the data at infinity thanks to the uniquely defined flow in 5.

Another way to test the validity of such a theory is to notice that data for various stars must be fitted as explained above with the same interstellar medium.

### *Discussion:*

— A. J. DEUTSCH:

I should like to clarify several points. First, I think it is not quite correct to say that there is a controversy between PARKER and myself. As I understand the situation, there is not necessarily any controversy. We depart from different sets of observations, with different physical problems in view, and



attempt, each of us, to find the solution appropriate to his particular problem. It would be nice, of course, if we could relate these two. As I understand it, Parker's problem is this; he believes there is good observational evidence for a high temperature solar wind of the order of 200 particles per cubic centimeter, at one astronomical unit from the sun, moving with speeds of the order of 500 km/s. My observational evidence, on the spectra of the M-giants, relates to a situation in which I observe gas flowing outwards with speeds of the order of 10 km/s; I do not know what the density is; I do have some indication of what the temperature is, and it appears to be very low. So, there are major differences in the point of departure.

Consider again Fig. 8 from my paper, which summarizes the regimes that are possible so long as I restrict myself to the case of continuous adiabatic flows. The abscissa is the value at the base of the flow of the ratio of the escape velocity to the thermal velocity, and the ordinate is the initial Mach number. From this Figure I see that I have the option of going to flows which at the base are characterized either by very large flow velocities—that means large  $\beta_0$ , or by very high temperatures—that means large  $(V_{th})_0$ ; or both. If, for example, I go in the direction of low initial thermal velocities, so that the gas is cool near the surface of the star, then I must move to the right in this diagram. But I find that I cannot go very far in that direction in the subsonic regime before I run into the region where no continuous flow is possible, and I am therefore forced up into the supersonic regime, where the initial flow velocities are at least comparable with the escape velocity; and I say that the observations of the M-giants do not admit this possibility. For if I have a cool gas, which is expanding from the star with flow velocities comparable with the escape velocities, I must expect to observe this, and I do not observe it. And therefore I take the only option which is left to me within this framework, which is to say; there may be a high temperature region around the star, sufficiently high in temperature that I cannot observe it. Let's see how well I can get along, by moving off more to the left in this diagram, and simultaneously attempting to keep the flow velocities as low as I possibly can.

Return to Fig. 10, (page 254), which illustrates the characteristics of a typical supersonic adiabatic flow. Notice here that the initial flow velocity is almost equal to the escape velocity; but the initial temperature is rather high, so that I need not to be too disturbed that I fail to observe flow velocities that are comparable with the escape velocity. However, I find that by the time I get out to 10 stellar radii, the flow velocity has risen to about 150 km/s. The temperature is still too high here for me to observe the gas. But the flow velocity stays high; it has already essentially reached its asymptotic limit, and beyond  $x=10$ , I must expect the velocities to remain high, right into the observable cool region. This clearly is inadmissible from the point of view of the observations. To the best of my knowledge, all of the supersonic adia-

batic flows will have this characteristic, and are therefore inadmissible in attempting to explain the observations of the M-giants.  $\gamma$  is  $\frac{5}{3}$  here.

Now, in endeavouring to discuss the subsonic adiabatic flows, I asked myself whether one can limit the possibilities to flows which will merge smoothly with the interstellar medium—that is to say, in which the velocity goes to zero, as indeed it does in all of these subsonic adiabatic flows, and where simultaneously the temperature and density go to the values appropriate for the interstellar medium. And I find that I can do this, if I limit the possible range of values of the initial Mach number and the ratio  $(V_{es}/V_{th})_0$ . Fig. 11 (page 255), again reminds us of what those limitations are. I find that, corresponding to a given escape velocity, the initial flow velocity may have any value which lies below the line labeled  $V_0$ . The initial temperature, however, is very closely prescribed; it must lie on the dashed line if the initial Mach number is zero, and it must lie on the full line if the initial Mach number is 1. Moreover, it must lie between these two lines regardless of the value which is assigned to the temperature of the interstellar medium, whether this temperature be 100 or somewhat more than 10 000°. Similarly the initial density is closely prescribed by the outer boundary condition. At this point, for the first time, I noticed that at the abscissa 2.5, which corresponds to the escape velocity at four solar radii from the center of the sun, the diagram predicts a temperature of 3 million degrees, and a density of  $3 \cdot 10^6$  protons/cm<sup>3</sup>. Since these numbers correctly represent the solar corona within a factor of 2 or 3, I naturally wondered whether the same notions, which I was developing in the context of the M-giants, also have an application to the case of the solar corona. I throw this out as a question. Certainly, if we go to this kind of an interpretation, then we cannot reproduce the high velocities for which PARKER believes there is good observational evidence in the neighborhood of the earth.

Now, I have considered in most detail a case where the initial velocity is 44 km/s. However, since the initial temperature is 270 000°, I probably will not be able to observe the gas at the point where it moves with this velocity. I find that in its subsequent motion the gas quickly decelerates, until by the time I've reached 100 stellar radii, the velocity is down to 2 or 3 km/s. I notice that the gas always moves with a velocity less than the local escape velocity,  $V_{es}$ , which is given by the straight line. To find whether a flow of this kind is consistent with the observations, I compute the projected density of Ca II in the line of sight, taking rough account of the ionization gradient in the flow; and I also compute the mean expansion velocity along the line of sight. The computed surface density turns out to be less than 1 percent of the value observed in the red giants; and the computed expansion velocity is also small, by a factor of nearly 10. It must be noted here, however, that there is a grave question as to whether one can give any physical justification

for assuming that the flow approximates adiabasy. The one investigation of this question that has been made, by R. WEYMANN in his thesis at Princeton indicates that radiation causes gross departures from adiabasy, at least for the case of flows which are somewhat more dense than the one which I considered here. In any case, it looks as though most of the M-giant stars are losing mass at a rate which violates the limit set by the nozzle that we were talking about earlier. It looks very much as though we have rates of mass-loss which exceed by several thousand times what this kind of flow can give.

— E. SCHATZMAN:

I think that the adiabatic condition is too correct to obtain the right dependence of temperature, density, and pressure in these layers, and that the difficulties you have with the late giants can come from that.

— A. J. DEUTSCH:

I have the impression from Weymann's work that the difficulties become more severe as the density goes up. The difficulties are apt to be much less in the case of the early M-giants. I'm not sure that the adiabatic approximation is too bad there, but when the density goes up and the radiative processes take over, then they remove the basis for this whole picture.

— R. LÜST:

I want to put a question. Would it not be possible that these kinds of stars would be surrounded by a hot corona? In this case there might be no such difficulties with the mass-loss; and the same picture which PARKER has applied to the sun, would then be applicable for these kind of stars.

— A. J. DEUTSCH:

Yes, I think it is. I think that at the base of flow which I just described there is something that approximates a corona. The temperature may be something like  $300\,000^\circ$ ; one does not need a temperature as high as 1 million degrees. I should like to know whether, starting with a very much higher temperature—say a temperature ten times higher—one can contrive to keep the velocities in the observable region down in the range which I observe. I'm unable myself to see how to do this, particularly if I have to go to supersonic flows at large distances.

— P. GERMAIN:

May I ask DEUTSCH if he can show us which part of the integral curve he has considered, especially where he locates the initial value of  $R$  in the diagram. It is the subsonic part which is after all the critical value of  $R$ .

— A. J. DEUTSCH:

The difficulty is that Germain's discussion breaks down for the particular case I have discussed, where  $\gamma$  equals  $\frac{5}{3}$ . I said that the critical point moves off to infinity. It does not, it moves off to zero. I think the curves can always be divided into two classes, can they not? In one class, the velocity always decreases monotonically and goes to zero; in the other class, the velocity increases monotonically. I'm not sure of the latter. I think it goes logarithmically to infinity.

— W. V. R. MALKUS:

The process of accretion has been touched on occasionally in the symposium. Could DEUTSCH comment on any evidence that stars in the denser regions of the interstellar gas have spectra which may indicate inflow? Possibly CLAUSER or PARKER could comment on the inflow case?

— A. J. DEUTSCH:

I think a fair statement of the case would be that we are pretty much guided in the choice of the theoretical problems we investigate by the observational problems with which we are confronted. There are sound theoretical reasons for believing that stars must indeed be condensed out of the interstellar medium; and we know some places where we think we can see this happening. The details of this process, however, are extremely small. But in recent years we have been confronted with a lot of evidence that indicates that we can see before our eyes a wide variety of stars spraying matter out into the interstellar medium. Therefore the emphasis has been laid, I think reasonably, upon these problems. I think the statement made earlier, about our having little or no evidence for seeing matter fall into stars, is correct. This must happen; but it's awfully hard to observe it.

— N. MILFORD:

I would like to address a simple question to the aerodynamicists. Would it make any significant difference if the boundary conditions at infinity change, as they do, because of the variations in density and velocity of the interstellar matter? If these changes occur in a time of order 1000 years, would there be any significant feedback into the inner regions?

— H. LIEPMANN:

I think that we should actually ask the question a little broader. We have discussed so far only stationary solutions; and the problem cannot be stationary, I think, since probably you have explosive formations on the surface of the stars, and you have changes at infinity. So, if you like, we should maybe



take a few minutes and discuss the possibilities of non-stationary outflow, or influence of non-stationary conditions at infinity on the outflow.

— R. B. LEIGHTON:

Also, will it really be true that the flow can ever be isotropic, because the star is moving through the interstellar medium?

— A. J. DEUTSCH:

Certainly these complications require consideration. However, the observations suggest that they probably represent second order effects. That is, there is observational evidence, supported by theoretical arguments, that the processes we are considering here are quasi-stationary. I think we should expect to be able to give a fairly good account of the observations in terms of a stationary theory. But there may be some very interesting spectroscopic problems relating to non-stationary problems.

— H. PETSCHKE:

Could one give a criterion for when he can treat the flow as quasi-stationary? I think this would be when the time it takes the particles to go through the flow field is shorter than the time in which the boundary conditions change.

— A. J. DEUTSCH:

That condition is satisfied. You see, at 10 km/s, matter moves 10 parsec in one million years. The average distance between the stars is of the order of 1 parsec.

— N. MILFORD:

I don't think that there is general agreement that the fluctuations in density of the interstellar medium are necessarily of the same order as the distances between the stars; we don't actually know what the scale of the density fluctuations is. In previous meetings of this series we have had several different scales given for these fluctuations.

— F. KAHN:

I would have thought that the scale of fluctuations was very much larger than the distance between the stars, with clouds possibly 5 parsec across and maybe 100 parsec apart, so, in fact, if you make the scale of fluctuations about 1 parsec you are making a gross underestimate.

— F. H. CLAUSER:

I think that I can give a statement as to what to look for in this non-stationary case. You can see this in your own washtub, if you allow the water

from the faucet to strike a flat plate there. You will find it goes down a column, and spreads out, and reaches a supersonic water velocity. You find that the flow spreads into a very thin, high speed layer; and then out at a certain distance, it goes through a shock-wave, in which the height of the layer increases manyfold, and the velocities become very low. This is my picture of what happens in the stars, that you get this supersonic outflow, and that out at a great distance there is a shock-wave, that converts the flow back to a higher density, higher pressure, lower velocity flow. Now you ask what happens if I begin to disturb something in the interstellar medium; and in particular, what would happen, if one gave a flow to the interstellar medium. All that happens in the bath tub analogy is that the circular ring that forms will be distorted; if you bring water in from this side, the ring will move over. The supersonic portion will be absolutely unaffected, and have no knowledge that any of this has happened; so that the entire set of boundary conditions given by movement of the shock-waves and all of the interstellar discontinuities, non-stationary effects, etc., will be reflected by a movement of the shock-wave. So anything that you do out in the interstellar medium will have no influence on the supersonic flow—it is effectively isolated by the supersonic flow, and there is no way that things can move upstream. The entire change, due to the presence of the non-stationary effects and fiddling in the interstellar medium, will appear to be a movement in and out of the shock-wave boundary.

— A. J. DEUTSCH:

Let me ask two questions: First, whether in the astronomical context it's possible to give now some estimate of the order of magnitude of the radius of the standing shock-wave; and second, would you expect that this shock would lead to any observational consequences?

— E. N. PARKER:

You can estimate the shock position by the following argument. Coming out from the sun is a flow that has essentially constant velocity after about  $(20 \div 30)$  solar radii. Thus, density falls off as  $1/r^2$  and can be computed from its estimated value at the earth. The condition giving the shock mentioned by CLAUSER is simply that the pressure of the solar wind after passing through the shock must balance the interstellar pressure. The pressure across the shock is essentially  $\rho v^2$ . Take a velocity of a few hundred km/s and a density of some  $10^2$  particles/cm<sup>2</sup> at the earth. If the interstellar pressure is  $10^{-14}$  dyne/cm<sup>2</sup>, the radius of the shock is 5 000 a.u. If we introduce a magnetic pressure—which BIERMANN suggested might be a factor of  $10^3$  higher than the gas pressure—the radius is reduced roughly by  $10^3$ , or to about 160 a.u.

The observable consequences of such a shock are probably not visual, be-

cause the temperature is very high beyond the shock. I think that the most important consequence of the shock is its cosmic ray effects. The cosmic ray intensity in the inner solar system, during the years of solar activity at least, is rather low compared to the intensity in interstellar space. And this is apparently due to outward convection of cosmic rays in disordered magnetic fields, which occurs probably on the near side of the shock boundary as well as beyond. Thus, the 11-year cosmic ray intensity variation observed at earth probably originates in part at the shock transition and beyond.

— A. J. DEUTSCH:

In the case of standing shocks around stars, is there any hope that one might attempt to relate these to the generation of non-thermal radio noise?

— E. N. PARKER:

I am sure one would find a relation, because you would generate a lot of high-energy particles as a result of such a shock. The point is, that at sunspot minima, one observes a cosmic ray intensity which is high relative to when the sun is active. Cosmic rays come from outside the solar system except for brief intervals when the sun generates a few. Moreover, whatever is depressing the cosmic ray intensity must lie well beyond the orbit of the earth because there is no gradient at the earth. Finally, the only way one can exclude cosmic radiation is with a magnetic field. So, we conclude that the sun does something to depress cosmic rays, and we note that the shock just discussed occurs well beyond the earth orbit and will be the first thing encountered by the incoming cosmic radiation, by way of disordered magnetic fields. Any magnetic field will have some discontinuous configuration across the shock. To settle the cosmic ray problem completely, one must relate everything that goes on in the way of disordered magnetic fields from outside the shock clear through into the inner solar system.

— L. DAVIS:

There are actually two things that I want to say. First, of course, this question of what happens in the region around the sun as the gas flows out towards the interstellar magnetic field is a complicated one which has been discussed by cosmic ray physicists for about five years. There are a variety of models—PARKER says probably none of them is right—but some of them are more right than others and one can combine features from them. There is one thing in the model that he mentioned that I think I wanted modified. As the activity of the sun changes, the position of that shock front is going to creep in and out, just as Clauser's ring moves in and out when you turn the tap on and off in your bathtub. This changing volume of the region in

which the cosmic rays have difficulty in penetrating will also produce effects. Another reason that the cosmic rays have difficulty in getting into the region near the sun, in addition to the difficulty of diffusion through disordered fields, is just the fact that the cosmic rays in the galaxy are going along the galactic lines of force which don't come into the region inside the shock-wave at the interface between the solar wind and the galactic field. They can easily get into this region only at the ends where the galactic field splits or along irregularities. Well, I say that this indicates that the model is more complicated—which of course PARKER knew from the beginning as he told us.

The other point concerns what one might think would happen when this solar wind comes out and strikes a magnetic field. And here I come back to the point of view of which I seem to be the sole representative—of looking at the information that one gets from satellite observations. Let us consider not a galactic magnetic field against which a solar wind blows, but rather the earth with its dipole field, and let us look to see what happens in the region where we think the solar wind is blowing on the earth's dipole field. We find that in 2 satellites—one which went out within  $10^\circ$  of the earth's sun line and one which went out within about  $45^\circ$  of the sun line—there is evidence of the same thing happening. Unfortunately the evidence is not clear enough so you say precisely what happened but you can give some idea of what it is. First, the solar wind did not push the geomagnetic field in as far as one would have expected from the simple momentum balance that PARKER gave. Correspondingly, one would think that it might not push the boundary

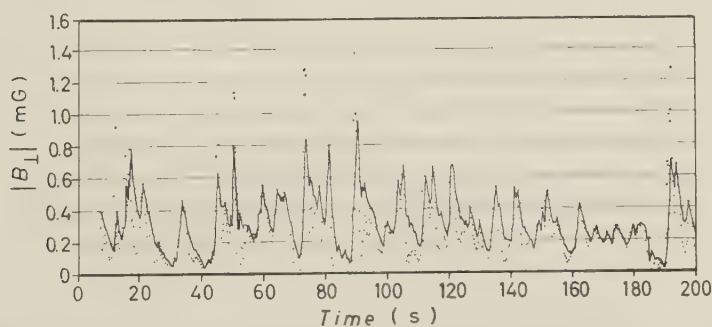


Fig. 5. — The component of the geomagnetic field normal to the spin axis of Pioneer I in the region of 80 000 km from the center of the earth.

between the solar wind and the galactic magnetic field out as far as the momentum balance would lead one to expect. The second thing is found in the rather thick boundary region between 80 000 km from the earth's center and 120 000 km, where the last trace of the dipole field is found. In this region there were very irregular magnetic fields as shown in Fig. 5, which is taken



from the work of Dr. C. P. SONETT and his collaborators at Space Technology Laboratory. It shows the magnetic field plotted against time, as observed by the satellite. The expected dipole field that one would get is about 30 gamma, that is about  $30 \cdot 10^{-5}$  gauss. That is about the average field, I think. What one sees is a field that has wriggles in it. The time scale is something like 10, 20, 30 s between peaks. It falls down to very much less than 30 gammas in some places. This could be a geometric effect since the satellite measures only the component of the field normal to its spin axis and, if the field varies in direction, you could observe only a small component of a large field at some points. These peaks though will go up—some of them to 100, many of them to 60 or 80 gamma. Many of them are quite symmetrical, many others start out to be fairly symmetrical but show a filling in on the back side. These peaks do not seem to be stationary structures, existing in the outer atmosphere of the earth as a constant high magnetic field in a little region. This just seems impossible. They look more like waves, probably propagating inward—if they are propagating inward they have slightly steeper fronts than backs. They have more resemblance to a symmetrical solitary wave than to the classical shock, which I suppose one would think of as a steep front and fairly flat back. The times of sharp rise and fall are a few seconds. It would appear then that these waves transfer the momentum of the solar wind across weak fields rather than blowing the field away; they probably also carry substantial amounts of energy. Perhaps a little later on it may be possible to say more about what the nature of these waves is. But I think one can regard this as a kind of a laboratory scale observation of some phenomena which probably have importance in many of these astrophysical situations—if one only sees how to transfer it.

— H. LIEPMANN:

It looks to me as if this is one case where you may infer that the non-stationary problems are important in discussing the solar wind.

— F. KAHN:

I think that the question has been raised whether one is justified in talking about a hydrodynamical approach to the question of the outflow of material from the sun—whether an adiabatic approximation is good in that case or not. And I think one can find some numbers to show that there is nothing much to worry about. Let us first consider whether the mean-free-path is small compared with the scale of the motion. If we start with a completely ionized gas, we get a collision cross-section of this form

$$\sigma_{\text{collis}} \text{ is of the order } \frac{\pi e^4}{(kT)^2} \log \left\{ \frac{(kT)^3}{4\pi N e^6} \right\},$$

$N$  being the particle density. Now in the case of the sun, starting from a place where particle density is  $10^7$ , temperature = one million degrees—we find a collision cross-section which is about  $\sigma = 3 \cdot 10^{-16} \text{ cm}^2$ , and again putting at  $N = 10^7$  we find that the mean free path is by two orders of magnitude smaller than the radius of the sun. Since we probably start considerably further out than at the surface of the sun, this seems to be entirely satisfactory.

Now the next point is, how does this vary in any reasonable flow? And we find that the further away we go from the sun the more satisfactory our approximation gets, because we have that, apart from a log factor which does not change too much, the length of the mean-free-path varies as  $(Nk^2T^2)^{-1}$ , which is proportional to  $1/Na^4$ . In the adiabatic flow of a monotonic gas,  $Na^3$  is constant. Thus, the mean-free-path varies as  $a$ , and its ratio to the typical length scale of the flow,  $R$ , varies as  $a/R$ .  $k$  = Boltzmann's constant,  $a$  = sound speed,  $R$  = distance from sun. Now you see, as we go away from the star, that we enter the region of supersonic flow ahead of the shock, and here the sonic speed drops all the time;  $R$  increases all the time. In fact, the approximation gets better and better, provided we are considering motion inside the stream. Of course, if the stream runs into another mass of gas, the mean-free-path for the collision will be determined by the relative velocity of the encounter and our mean-free-path formula would be wrong.

The next point is, can the gas stay ionized? There is a very rough formula for the rate of recombination of electrons with hydrogen ions which is valid—more or less—in a reasonable temperature range. The rate of recombination is  $5 \cdot 10^{-11} n/\sqrt{T}$  recombinations/s per ion, where  $n$  is the electron density,  $T$  the temperature in  $^\circ\text{K}$ . At the surface of the sun, or at least in the region where the stream sets out, the recombination rate works out to be  $5 \cdot 10^{-7} \text{ s}^{-1}$ . You would therefore have to wait about  $2 \cdot 10^6 \text{ s}$  before a given particle recombines if it stays in the corona. This is about a month, and, of course, much longer than the time it takes for a particle to get away from the region. As time goes on the rate of recombination goes as  $n/T^{1/2}$ . Once again  $n$  varies as  $a^3$ ,  $T^{1/2}$  varies as  $a$ , and  $n/\sqrt{T}$  varies as  $a^2$ . The speed of sound keeps decreasing and therefore the recombination rate goes down and down. We are interested, in fact, in the recombination rate compared with the time a particle spends in a given region. This time is given by the distance from the sun divided by the speed of the stream. The ratio that we are after is thus  $a^2R/u$ ; finally we have from the condition of continuity, or flux condition that  $NuR^2$  is a constant of the motion.  $N$  again is proportional to  $a^3$ , so that  $a^3uR^2 = \text{constant}$ , and we find that  $a^2R/u$  varies as  $a^{1/3}/u^{2/3}$ . Now  $u$  increases to a constant value; and since  $a$  again keeps on decreasing, the recombination rate multiplied by the time scale typical of the motion also keeps on going down. Thus, if the gas doesn't recombine while it is near the solar corona, it is never going to recombine at all. This also excludes the possibility that the sun raises

the temperature of the gas by photoelectric heating, the particles just don't recombine—so you can't heat them up by ionizing them again. The only thing that one might have to consider is whether waves from the sun, such as are supposed to heat the corona, can travel out further into the gas when it is moving away from the sun. But I don't want to comment on that.

— H. LIEPMANN:

A comment on one point which SEVERNY brought up; namely, the question whether radial outflow of this type can be considered hydrodynamically stable, *i.e.* whether you expect radial velocities only, or whether you expect in a problem like this to get velocity components in a non-radial direction. Has anybody a strong opinion on this? My own opinion would be that it is stable—I think that radial outflow is stable and radial velocities dominate except of course during explosive processes like that on the sun.

— S. GOLDSTEIN:

Not only is a spherically symmetrical flow with a shock stable, but a flow which is non-symmetrical to begin with will, in a short time, approach symmetry.

## PART IV.

### Considerations on Localized Velocity Fields in Stellar Atmospheres: Prototype — The Solar Atmosphere.

#### A. - Convection and Granulation: Preview on Granulation — Observational Studies.

---

*Chairman:* E. BÖHM-VITENSE

— E. BÖHM-VITENSE:

This afternoon we consider the photosphere of the sun. It is generally assumed that most of the velocity fields which we observe in late-type stars (stars with low effective temperature  $\lesssim 1 \cdot 10^4$  °K) can be traced back to the hydrogen convection zone, which occurs, in the case of the sun, in a layer at a depth of about 400 km. I mention the sun because it is the only star where we can really see the effect which the convective layer has on the photosphere. We are going to see some films about these effects, which are observed as the phenomena called granulation. We will observe layers ranging between about 400 km and about 250 km as the observed region moves from center of the disk to the limb. Finally, note that with our instruments on the surface of the earth, we can usually resolve an area about 1 second of arc due to the limit of seeing in the earth's atmosphere; this corresponds to a distance of 700 km on the surface of the sun.

#### J. RÖSCH: Observations from the Pic du Midi.

As an observing astronomer, I have been very much interested in the high degree of completeness of the theories, and in the difficulties arising in reaching an agreement between different theories. I hesitate to bother both the theoreticians and the aerodynamicists with this other major problem, which is to get an agreement between theory and observation. But I will show you some examples of what may be done by trying hard under the best possible conditions, and show you the limitations which we have in the observations. From these observational results you will see, I hope, what limitations you



have on the theories; and the limit to what we know from the observational point of view, so then you are completely free for theory.

In order to show you some examples of the limitations in the observation of the solar granulation—I shall first show some slides before the film. The first was obtained at the Pic du Midi Observatory under good atmospheric conditions with an objective of 23 cm aperture; we believed for a long time that this was the best we could obtain. This slide is <sup>(1)</sup> to allow you to compare these with subsequent results obtained with larger objective. With this type of picture one may evaluate the average distance between the center of granules, then compute the mean diameter of the granules, make autocorrelation functions, and so on. We had the impression that something better could be obtained with good atmospheric conditions, because on occasion we had some details finer than these. The next slide shows a sunspot with umbra, penumbra, and granulation on the average photosphere, taken in the same conditions. The next slide shows the same spot but printed in such a way that one sees inside the umbra small granules, smaller than those on the photosphere; we had the impression that these granules were below the limit of resolution of our instrument but rather separated from one another and bright and small. We had also photographs showing the granules near the limb of the sun, and you see that granules appear elongated parallel to the limb within a few seconds of arc of the limb, and with a width which is definitely smaller than 1" of arc. Next is just the same type of thing with elongated granules. Then we come to another type of record (Fig. 1), which is obtained always at the Pic du Midi with an objective of 38 cm aperture (theoretical resolving power of about  $\frac{1}{3}$  of a second). This photograph has not been taken in white light, but in orange light, in a bandwidth of approximately 100 Å between 5900 and 6000 Å. We here have a decidedly different type of thing. From our first results we thought it was possible to define something like an average granule with an average size and so on. Here it appears much more difficult, if not impossible, to define an average granule, because we have big circular granules, other elongated small ones, and so on. As definition is improved, things become more and more difficult to interpret. We have not yet succeeded in taking simultaneous pictures of the same field on the sun at different wavelengths, but I hope we soon will get such pictures. The structures have roughly the same appearance in differing wavelengths, but probably the contrast between granules and intergranular spaces is different. The next slide demonstrates the method used long ago to obtain a determination of the lifetime of the granules, which is, of course, an important element in the granulation. It is what one calls a « moiré » picture, obtained

---

(<sup>1</sup>) Almost all the slides presented and not reproduced in the present publication may be found (except the « moiré » effects) in: *L'Astronomie* (1957), p. 129.

by superimposing two pictures, but not exactly identical. If you slightly move these two pictures with respect to one another, then you find some patches or series of things like that. You have either rows, straight lines or arcs de-



Fig. 1.

pending upon the direction of the displacement of the two pictures. As long as you have « moiré » effects on such pictures, you can say that there is something in common to the pictures; this was used for instance by GROTRIAN,

TEN BRUGGENCATE and others to have an estimate of the lifetime of granules. The next slide has been obtained by superimposing two images taken at  $2\frac{1}{2}$  minute intervals in time; the preceding one was obtained with two plates at 5 minute interval. The effect is much more evident on those at shorter interval, but it is still evident at 5 minutes, and this may give an idea about the lifetime of the granules. Our experience showed that it was difficult to have an accurate estimate by this method. When we increase the definition of the pictures, and the details in each granule become more important, then it is still more difficult to obtain «moiré» effects. We found that really the only



Fig. 2.

way to have some idea about the evolution of granules was to try to have good resolving power and compare the granules one by one, compare several images of the same granule, and the best way to do that, if possible, is to have moving pictures. This is not easy.

Fig. 2 is a way of representing the results of granulation. It is obtained with two images of the type which you have seen in orange light, but it shows not the *brightness* of the granules but the *gradients* in brightness. It is obtained



by superimposing a positive and a negative of the same image; it may be shown (this has been worked out by Mr. HUGON, one of my assistants) that what appears is the gradients of density on the negative. It is amazing to see how clearly the details appear; this technique may be useful for some studies on the granulation. The next slide is an example of what we have picked out of a series of images obtained during a number of minutes. It shows that quite often a granule changes in form, in shape, and in size with time. One granule splits just as, let us say, a living cell or a coffee grain. For instance, the lower part, which becomes a triangle, splits in three parts and here it has almost disappeared. The next slide is a series obtained at intervals of 1 minute of time. Fig. 3 is another series covering 18 minutes and several features may be remarked on this. For instance, (*a*) splits and almost disappears—here (*a'*). The small one (*b*) is a part of an old granule. It will increase gradually here (*b'*), it is bigger, and here (*b''*) again it increases and gets bigger and bigger, and here (*b'''*) it begins to split in several small parts. Look also at these two there (*c*, *d*). You may follow them all in this interval of 18 minutes but with some sign of splitting here (*c'*); this one increases, you see and takes a shape like fingers of the hand with three fingers here (*d'*)—here you see one, two, and three fingers—then the fingers divide (*d''*) and so on.

After these slides I have just to show you the film. I must explain to you how to look at this film. The meteorological conditions in our situation are generally as follows: a mountain peak 9200 feet high, rather isolated from the other mountains in the surroundings, and this may be one of the reasons for which the conditions are good. We observe the sun when it rises in the morning, and then the images are not very good because the light goes through a big mass of air. Then the conditions improve and at a certain moment we have good images; not on all days, but fortunately sometimes. Then later on, after 1 or 2 or 3 hours, we have nearby turbulence caused by the warm air going up along the slope of the mountain. We found on this film random motions of the granules which we suspected to be due to the atmospheric turbulence, because, as I said, the film was taken low on the horizon (between 15 and 20 degrees) so that we have remote turbulence which may not affect the sharpness of the images but make a sort of distortion. In order to know what was due to these atmospheric effects, we selected one of the sequences which was good enough to give us about 2 dozen frames of a sharp definition taken within 3 s of time. With this, we have prepared another film, which will be seen first, on which one sees only the atmospheric effects because we can make the assumption (and I think this is probably true) that during 3 s of time we have no change on the sun, at least not within our definition. Then on the second part one sees terrestrial effects plus solar effects. So, on the first part, granules have a sort of random Brownian motion, and there is a sort of pumping of the granules, a granule increasing, decreasing, and so on;



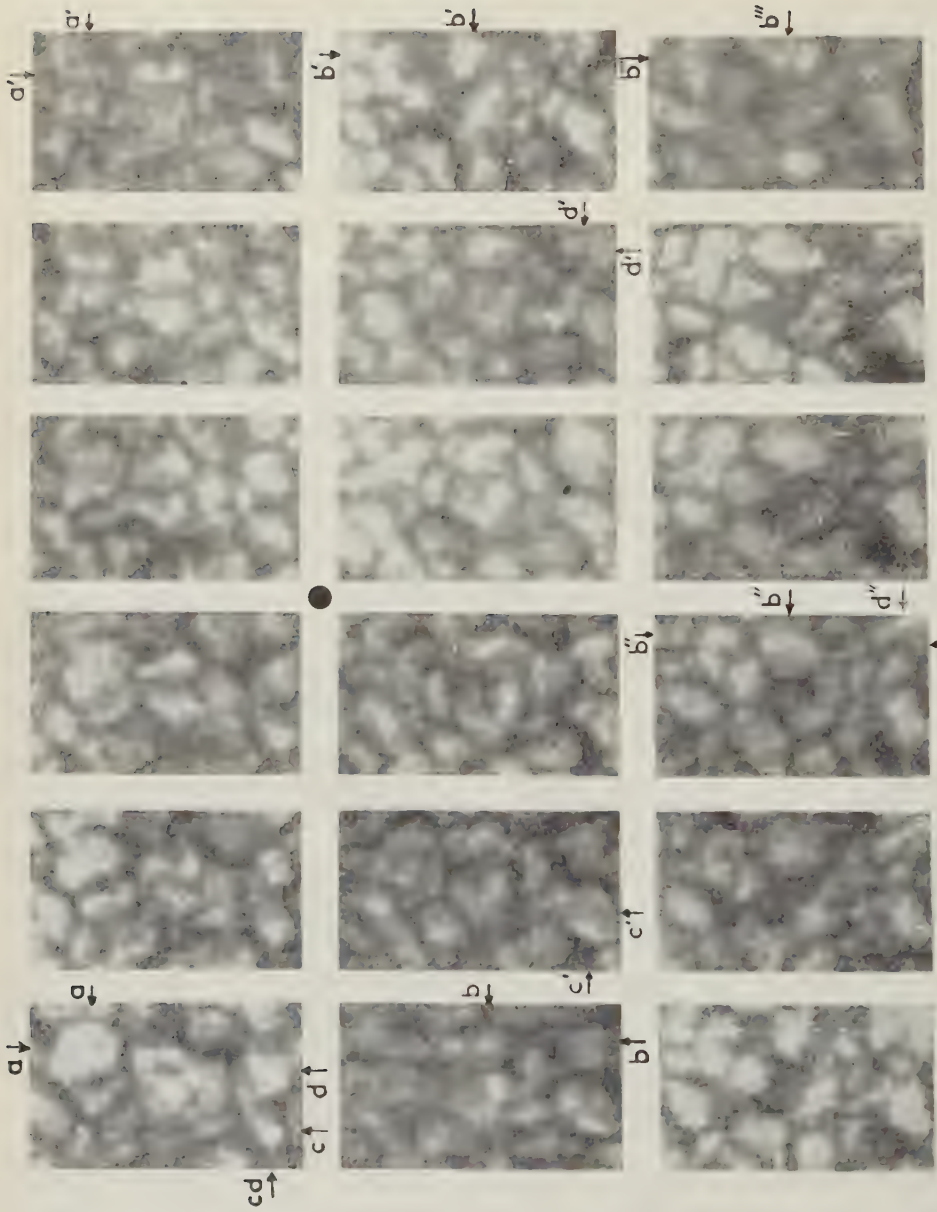


Fig. 3.

but may I say that a big granule remains a big granule, a bright one remains a bright one and so on. On the second part you will see these effects plus others; and these others are mainly sorts of splittings of the granules just as you have seen on the slides. They give the impression that it is an irreversible phenomenon. This is my impression. Maybe you have another one. Quite often there is a dark point in the middle of a bright granule, and I have been very pleased to see the same dark points on the Princeton photographs.

As a conclusion, may I point out that the program, which we are carrying on since 1954, and which has produced some new results of importance (granules near the limb, 1955; intra-facular granules, 1956; evolution of the granules, 1956; evolution of the granules, 1959) takes its efficiency from the fact of being a long-range program, taking profit of its ground-basis which permits changes in type of observations, operating conditions, and so on. It is very fortunate that other groups have been able to tackle the problem by quite different ways—and with converging results. But I am sure that due to the flexibility of our program, we are in a good position to collect some of the data still so eagerly needed for interpretation of the photospheric phenomena.

#### E. SPIEGEL: **The Princeton balloon observations.**

As you have seen, the pictures of the solar surface until now revealed a two-dimensional random brightness field. In spite of the required visual appearances, at the one-second resolution limit, microdensitometer traces in the two-dimensional plane have given every appearance of a random brightness field. Now it has been felt, of course, that the statistical properties of this field would be the most direct and relevant clue to the nature of the fluid dynamical activity in the underlying hydrogen convection zone, and so it has been considered of pressing interest to determine as well as possible the statistics of this fluctuating brightness field. At Princeton some years ago it was therefore undertaken to send instruments to a distance above the tropopause where most of the atmospheric motions seem to be taking place that cause the seeing difficulties. And so some years ago a 12 in. telescope, 30 cm aperture, was sent aloft to 80 000 feet (24 km) to take photographs of the solar surface. Four flights were made at that time, and the results of these observations have been published by SCHWARZSCHILD (*Ap. J.* in 1959) as well as the relevant instrumental details. But only single pictures at certain moments turned out to be good enough for the kind of analysis one was interested in. What one found there is that the r.m.s. brightness fluctuation is of the order of 5 percent, this corresponds to about  $80^\circ$  fluctuations in the solar photosphere if one assumes black-body emission. The arguments for this are published in the paper I referred to. However, I would like here to discuss results from the most recent flight which was made last summer; that

is, four flights made last summer, in which it was possible to maintain good focus over a long time interval and thus to obtain data relevant to time lapse studies. I should say at the outset that I have no connection with any of these activities, my connection with Project Stratoscope, as it is called, is only during the time of this meeting, so that I may show the film which was taken under the direction and leadership of SCHWARZSCHILD. The active participants at the moment are also BAHNG, DANIELSON, and ROGERSON. I would like to report on these two aspects of the studies made from these most recent flights. One is a statistical study of the time-dependence of the brightness field at the surface. If we consider that the brightness field is given by some function  $B(x, y, t)$ , where  $x$  and  $y$  are co-ordinates on the solar surface, BAHNG and SCHWARZSCHILD have studied the correlation defined in this way:

$$R(t) = \frac{\int B(x, y, t' + t) B(x, y, t') dx dy}{B^2}.$$

I will show you the results of their measurements, and then I will show you the film which has been prepared by DANIELSON on the activity of sunspots.

First slide: It is this kind of material that has been used to evaluate the auto-correlation in time. This shows the same region of granulation at an interval of  $2\frac{1}{2}$  minutes.

Second slide: This shows the evolution after 5 minutes of a region. You can still see, of course, granules persisting; and one can follow some individual granules for quite a long time it turns out, perhaps 20 minutes, depending on whether you think the granule is as it was. It distorts considerably, but you can recognize the original entity which is the granule. And, of course, one striking feature is that it seems that the bright regions are greater in area than the dark regions. However, this, of course, depends on how you set the brightness zero, and that is a ticklish point. But the microdensitometer gives the impression that there is more area on the bright region than in the dark intermediate region.

The third slide shows the auto-correlation as a function of time interval. There are two outstanding features: one is, of course, the extremely good fit of an exponential curve to the measures; and the second is the rather good agreement between the behavior near sunspots and away from sunspots. I think you will agree that the lifetimes are somewhat longer than have been suggested previously, certainly by the Potsdam observations. This is something one might have been surprised at, because presumably this is a higher resolution. I should mention that the granules observed are in the range of 300 km to about 1800 km, the 300 km being the lower limit of resolution. It is, however, felt from studies of limb darkening and so on that one is very close to observing the smallest features. This then is the summary of the results on the ordinary granulation. I don't want to discuss here the theoret-



ical aspects. The fourth slide shows the kind of detail one can get in the sunspots. In the fourth flight last summer, there was made a particular attempt to follow a sunspot. The fifth slide shows a very large sunspot. DANIELSON has given considerable thought to these structures, in particular trying to explain the filamentary character. I do not have time to go into his theoretical discussion, but he seems to have ruled out all possibilities one could think of or one has thought of anyway, except the possibility that this elongated filamentary structure is produced by convective roles. One feels that the prevailing magnetic field which emerges from the sunspot is horizontal in the region of the penumbra, and that this magnetic field inhibits the convection which would have arisen in the absence of the magnetic field. The inhibition gives rise to a new form of convective motion, which has been studied at least in the incompressible case (convective roles being the cause of this pattern) although I am not at liberty to discuss it now because of time.

— R. B. LEIGHTON:

We have been spending about a week here discussing velocity fields, so I would like to take the liberty of showing you some as they appear on the surface of the sun. Let me first outline briefly the results which our observations have indicated to us. First, we have definite evidence for *horizontal motion* (i.e., tangential to the solar surface) whose magnitude lies somewhere in the range 0.2 to 0.5 km s. on a scale of about 30 000 km. This size is relatively large compared with the solar granulation. These motions represent relatively steady flow away from centers at which upward moving material arrives at the surface. There is some indication of a correlation with the emission in the *K* line of calcium. In addition, we find *vertical motions* which have a strong correlation between brightness and direction; namely, bright elements seem to be moving upward on the average—here the velocities are in a range 0.3 to 0.4 km s and the linear scale is about  $3 \cdot 10^3$  km and larger. The lower limit to the size is determined by our resolution—there may well be such motions on a smaller scale. These vertical motions show a strong oscillatory character, with a period of  $(296 \pm 3)$  s, based upon about 25 observations. The number of oscillations that a given volume element will undergo before the oscillation dies out lies somewhere in the range from 2 to 4.

Now as to the means of observation—this is similar to the scheme devised a few years ago for measuring the magnetic field (R. B. LEIGHTON: *Ap. J.*, 130, 366 (1959))—it is based upon a photographic cancellation procedure in which one simultaneously takes two photographs—(with the spectroheliograph) of the same region of the solar surface and introduces by suitable means a difference between these two photographic images, which difference is a measure of the quantity one wishes to study. We use for the most part a line of Ca I at wavelength 8103 Å, a relatively strong line so that the level in



the solar atmosphere to which the measurements refer is certainly something like a scale height above the photospheric level one sees in integrated light. We also use a line of Fe I at  $\lambda 6102$  very close to this. This has an excitation potential of about 4.8 eV for the lower state, and so is formed at a considerably lower level in the solar atmosphere. We have also made extensive observations in sodium  $D_1$  at  $\lambda 5896$ . Also, in passing, I mention that we have also made measurements in  $H_\alpha$ —these will have to be discussed at a later time. First, I show a slide which illustrates the principle of the method with respect to the magnetic field measurements, because to appreciate what comes later, one should know something of the procedure. We take a pair of photographs with the spectroheliograph—these are obtained by moving a slit slowly past a pair of solar images, formed using a beam splitter. It takes a few minutes to go from one edge of the image to the other. In the case of the magnetic field a quarter wave plate and polaroids are introduced in such a way that one photograph has blocked out of it the left-hand circularly polarized light and the other, the right-hand circularly polarized light. So, the difference between the two images is just what one needs in order to measure the line-of-sight component of the magnetic field. Now, in the case of the magnetic field, after having taken one scan across the image of the sun, we move the plate holder over, reverse the quarter-wave plate (which reverses the sign of the field sensitivity of the two images) and we then scan, in the opposite direction, back across the region we just came over. I mention this because we do a similar thing for the Doppler shift, and it plays an important role in detecting the vertical oscillations. In the case of the magnetic field, both of the images are taken using one edge of the line profile—and the quarter-wave plate and polaroid are introduced in such a manner that there will be a slight difference of intensity on the two images at any point where there is a magnetic field. As you see there isn't very much difference between these two images, and sometimes it takes a sharp eye to see that there is any difference at all. However, with careful photographic procedures, one can make a contact transparency of say, the right-hand pair of these images and develop it exactly to unit gamma in such a way that, if placed upon its own negative, it produces an essentially featureless field.

Now, if one places the contact transparency instead upon the other pair of images, the brightness fluctuations due to, say sunspots (wherever these fluctuations really are due *only* to brightness fluctuation *common* to the two pictures) disappear, whereas the true differences due to the magnetic field are doubled. As a final step we make enlargements of both of these «singly-cancelled» images to exactly the same scale, make a contact transparency of one of them and cancel it against the second one; this then removes the dust streaks which still remain at this stage and results in a «map» of the magnetic field of the region.

Now, for the Doppler effect we do precisely the same thing except we don't have a quarter-wave plate or a polaroid in the two light beams. We do, however, set the slit of the spectroheliograph on *opposite sides* of the line profile so that a shift in wavelength of the line will introduce an increase in brightness in one image and a corresponding decrease in the brightness of the other. The next slide shows the result of this method applied to an image of the entire sun—if we didn't already know it we would hereby have established that the sun *rotates*, because as you see the image varies smoothly from very bright at one edge, to very dark at the other edge. However, by twisting some knobs on the machine we can «tilt» the spectral lines and remove the part of the signal that is due to the rotation and leave only the signal due to random motions on the surface. The next slide shows the same thing with this having been done. Now here we see the first result that I mentioned earlier, namely, the appearance of what we take to be essentially *horizontal* motions on a large scale. You will notice that there is a «graininess» to the photograph predominantly about halfway from the center to the limb. There is hardly any Doppler signal near the center of the disc and, for other reasons, not much near the limb (because we are looking at such high elevation there and the resolution is not good enough to resolve them). Now look at the «grainy» regions; they are about 30 000 km in diameter, quite large compared with the granulation, but this is definitely a typical size as you can see. You will notice that they are always dark on the side towards the center of the sun and bright on the side away from the center, and they have an elongated shape which we take to be the effect of projection (because of the slanting view near the edge of the sun) upon essentially circular areas. Their absence near the center of the disc indicates that they correspond to *horizontal* motions which can only show up where you see them with a significant component along the line of sight. We believe these to be essentially outward motions, diverging from centers, presumably columnar convection currents, which bring material up from the convection zone, relatively deep underneath the surface, to the surface. You will see these on some of the further slides also. The next slide shows a pair of original images (at a larger scale) whose difference will eventually give us the Doppler pattern over the surface. The reason I show these is that one of the images has essentially *higher contrast* than the other. This is a reproducible characteristic always observed for two such images taken in the light of the calcium line  $\lambda$  6103. We take this as evidence for a *correlation between brightness and upward motion*. Consider the photograph which was taken with the slit set on the redward side of the absorption line: if we have a region which is intrinsically a little brighter than its neighbors and is also moving upward, (*i.e.* its absorption line is shifted toward the *violet*), then both for the reason that we have extra brightness—and also because the slit, being on the red side of the line, is brought more nearly into the continuum

by the violet shift, we get a greater signal at that point than at the corresponding point of the other image, where the two effects work in opposite directions.

The next slide shows the Doppler field on the solar surface at larger scale, and thus reveals motions on a finer scale. Here we see motions distributed more or less randomly all over the disc, except that they die out near the limb. This represents *vertical* motion, since we see as much of it near the center of the sun as part way to the edge, and possibly *horizontal* motion also. We can't separate them as yet. The vertical motions we see near the center have no typical dimension, but the elements go right down to a size that cannot be very much larger than that of the granulation. I would call the smallest size about 3 000 km, somewhat conservatively.

Now, as I mentioned before, in taking a photograph like this the slit of the spectroheliograph sweeps from one side to the other side in a matter of some 4 or 5 minutes. If, without changing anything in the apparatus, (we only change the plate to keep from getting a double exposure) we traverse right back again in another 4 or 5 minutes, we then have at this stage two similar, possibly identical, Doppler records. However, various things can make the two photographs different; one of them is the seeing, which is never perfect. Another is imperfect guiding. However, in addition to such instrumental or atmospheric sources of difference, if there are *accelerations* which change the velocities significantly within a few minutes' time—these should show up as differences also. To bring out such accelerations we take such pairs of pictures, and then cancel out one such photograph against its mate (taking a negative of one and the positive of the other). Thus, if there were no differences at all, one should get a uniform grey field with no feature in it at all. The result is shown on the next slide. Over on one edge, corresponding to zero time difference, we see relatively little signal. What signal there is, is a result partly of the seeing and partly because in making the adjustment of one plate on the other I purposely didn't quite cancel things out at this edge, in order to attain a better average cancellation over the plate as a whole.

Near this «zero» edge, as the time difference proceeds we see the growth of a signal; the middle looks quite different than the edge. That is not entirely due to the fact that we are closer to the center of the disc, but it is a characteristic feature of all such photographs. A velocity difference builds up, exactly as we would expect. Originally, we thought to measure the lifetime of granulation this way. However, we always found that there is a second origin, farther along the image, where there is sensibly less contrast in the signal than at either earlier or later times. This is the behavior which led us to the idea of an *oscillatory motion*. It always happens that the second region of good velocity correlation corresponds to a time difference of 5 minutes between corresponding points of the two photographs.



I emphasize that the effect is not merely due to a *shift* or distortion of the image. It is an intrinsic velocity change, an acceleration, which makes a signal which goes through maxima and minima. The next slide, which brings this out in another way, represents not a Doppler *difference* such as we just looked at, but what we call a Doppler *sum*; instead of taking two *similar* Doppler photographs, we take two which are intrinsically of *opposite Doppler polarity* and then cancel the positive one against the negative of the other—which is the same as *adding* the velocity at each point to the velocity at some time later. Zero time difference is again along one edge and we see that there is a very large signal, as we would certainly expect. However, the signal essentially *disappears* after a short time in the region near the center of the disc, and again builds up. We also see the larger cells out near the boundary of the sun which are long lived in a Doppler sum and don't ever disappear. The reduction of the signal near the center of the disc in a Doppler sum can only mean that the velocities have been reversed after a half period. We have many cases of this. It occurs very reliably, very reproducibly. We have changed the speed of traverse of the spectroheliograph and all the variables under our control, and it always shows up to one degree or another, depending upon the seeing. The average period, from 25 observations, is  $(296 \pm 3)$  s. The standard deviation of a single observation is about 15 s. The next slide shows a simple Doppler field taken in the  $D$  line of sodium. It shows the stabilization of the motions at a high level by the magnetic fields around the sunspot group.

The next slide shows the kind of a Doppler record one gets in  $H_\alpha$  by setting the slit rather far from the center of the line—about one Ångström—which means that one is looking at a relatively low level in the  $H_\alpha$  chromosphere. It can be described as consisting of large areas of essentially no Doppler velocity with « islands » of motion, little « funnels », through which the hydrogen gas is *streaming downward* into the sun. Out near the limb, of course, one sees both very dark areas and very light areas indicating horizontal motion which can be of either sign, but near the middle of the disk the predominant motion is downward through little tunnels. The tunnels shrink in size as one moves further out in the  $H_\alpha$  line. One also sees a few little spurts here and there in which there is *upward* moving gas also.

#### Discussion:

— G. ELSTE:

Which line was used to show this asymmetry in the motion?

— R. B. LEIGHTON:

Principally the 6103 of Ca—both for magnetic observations and the Doppler observations.



— G. ELSTE:

Did you try to do the same with the Fe line?

— R. B. LEIGHTON:

Yes—same results. I should also mention that, knowing what the period is, we arrange to scan the image of the sun over a smaller area in  $\frac{1}{4}$  of the period. We went  $\frac{1}{4}$  of the oscillation period in one direction and then another  $\frac{1}{4}$  of the period back again, and then  $\frac{1}{4}$  of the period out again and so on. Then in taking the second photographic difference we have the same time difference over the entire photograph, and this can be made either an even or odd number of half periods. We found that these are alternatively very contrasty and very uncontrasty. We can follow this difference in contrast out about three periods. This suggests that the  $Q$  is somewhere around 2 to 4.

— Mrs. BÖHM-VITENSE:

I think we can expect the main part of the line used to be formed at an optical depth for the continuum of about 0.05 or so.

— K. O. KIEPENHEUER:

In the picture there are black dots; have you thought of these dots being spicules?

— R. B. LEIGHTON:

I think there are not enough of them to be that.

— K. O. KIEPENHEUER:

They look exactly as they look in our filtergrams although I have, of course, not counted them.

— J. C. PECKER:

Did I hear you well—you did find a strong correlation between brightness and direction in the velocity?

— R. B. LEIGHTON:

Yes, at this level.

— J.-C. PECKER:

I want to draw the attention of the aerodynamicists to the fact that this is a very controversial point. In the continuum photosphere, many observers including the Michigan group, I think, found in some cases an anticorrelation. I would like to know what it means.

— G. ELSTE:

The Michigan arguments don't refer to this deep level.

— J.-C. PECKER:

Yes, all the observations do not refer to the same level.

— E. SPIEGEL:

Does that mean a correlation was measured or is this just a qualitative judgment?

— R. B. LEIGHTON:

At the present time it is qualitative, but clearly by measuring the contrast of the two pictures, which we plan to do, one can make it quantitative.

**A. B. SEVERNY: The motions and magnetic fields in the undisturbed solar atmosphere (outside active regions).**

The regular records of line-of-sight velocities with the aid of the solar magnetograph have been available at the Crimean Observatory since 1957, and a set of papers was published in *Publ. Crim. Observ.* since then. The method consists in recording the rotation of a plane-parallel plate before the slits of the magnetograph. This plate keeps the image of the line rigidly on these slits by equalizing the photocurrents from both slits, (the principle of the image follower). *The records of line-of-sight velocities* are always obtained *simultaneously* with the records of *magnetic field* because the principal aim of this image follower is to compensate automatically Doppler shifts of the line and to eliminate their influence on the measurements of magnetic fields. (To calibrate magnetic field in velocities of solar rotation we must switch off this compensator.) We found that there is no practical need to use a magnetic insensitive line (*e.g.* 5123.7) to record radial velocities, because of symmetrical change in Zeeman pattern at the modulation ( $0, \frac{1}{2} \lambda$ ) and high frequency of ADP modulation (120 Hz<sup>-1</sup>). But most records were made in a magnetic insensitive line, 5123.7 [1].

The main sources of error are 1) the turbulence in the *spectrograph* itself producing accidental errors, and corresponding to display of r.v. from  $\pm 50$  up to  $\pm 100$  m/s; 2) *the trembling of images* across the slit of the spectrograph producing at *bad seeing* accidental deviations up to 1 km/s. But this error is reduced (by repeating scans) to an amount which is less than the display owing to the turbulence in spectrograph.

A brief summary of our results relating to undisturbed disk is the following (the results were chiefly obtained by STEPANOV and partly by SEVERNY [2, 3]).

## 1. – Photosphere.

a) The velocity fields in the undisturbed photosphere are extremely complicated and as yet *we have failed to establish some regularity in them as well as to find any connection with magnetic fields*. The maps of velocities show no sign of regularity and stationarity (except active regions). The changes of magnetic polarity are not accompanied by changes of line-of-sight motions, as if the solar plasma were able to move freely across magnetic fields. This was confirmed also in the paper of STESHENKO [1], who tried to investigate the magnetic fields of granules, and also failed to detect the separate fields of granules exceeding 50 gauss, which is of the order of error of the best photographic measurements of Zeeman splitting with Rochon prism.

b) Some quasi-regularity still exists; and it consists in the existence of predominant ascending or descending motions over big areas reaching sometimes  $(2 \div 5) \cdot 10^5$  km in size. In these big zones we can find separate small regions with the sizes varying from 4000 to 20000 km, and of *opposite* direction of motion.

c) The maximal velocity observed in the photosphere (outside sunspots) may be estimated as  $\pm 450$  m/s. The mean square velocity was estimated as  $\pm 76$  m/s.

## 2. – Chromosphere (outside active regions).

a) The records were made in  $H$  and  $K$ -lines of  $\text{Ca}^+$  (by STEPANOV) and in  $H_\beta$ -line (by myself). They showed also complicated structure and the existence of big zones with predominant motions of one sign. The size of these regions is comparable with those of the photosphere, but a little smaller ( $\sim 2$  times).

b) On the background of these zones there exist separate regions with sizes ranging in the same limits as in the photosphere, and showing comparatively high velocities, exceeding sometimes  $\pm 3$  km/s. STEPANOV found that these regions are also moving irregularly *across the line of sight* with velocities reaching 5 km/s. The mean lifetime of these elements is  $\sim 7^h$ .

Above undisturbed regions with weak fields  $H < (5 \div 7)$  G, STEPANOV also found that the mean flux for ascending motions is equal to that of descending, and for these particular regions the mean velocities are  $-0.96$  km/s and  $+1.25$  for ascending and descending motions respectively.

Summarizing these results, we can provisionally conclude that in the undisturbed solar atmosphere there exist two characteristic sizes, and characteristic velocities of motions: large scale  $((2 \div 5) \cdot 10^5 \text{ km})$  and small scale (of the order of  $(5 \div 20) \cdot 10^3 \text{ km})$ .

## REFERENCES

- [1] N. NIKULIN, A. SEVERNY and V. STEPANOV: *Publ. Crim. Astr. Obs.*, **19**, 3 (1958); *Astron. Circ. USSR*, no. 183 (1957).
- [2] V. STEPANOV: *Publ. Crim. Astroph. Obs.*, **20**, 52 (1958); **24**, 25 (1960), in press.
- [3] A. SEVERNY: *Publ. Crim. Astroph. Obs.*, **24**, 288 (1960).
- [4] N. STESHENKO: *Publ. Crim. Astroph. Obs.*, **22**, 49 (1960).

— R. B. LEIGHTON:

I am not sure I understand what velocities correspond to the very large structures, the intermediate structures, and the granulation.

— A. B. SEVERNY:

The big structure corresponds to velocities of about 100 km/s, and the small structure to  $\sim 1 \text{ km/s}$ .



## PART IV.

### Considerations on Localized Velocity Fields in Stellar Atmospheres: Prototype — The Solar Atmosphere.

#### A. - Convection and Granulation.

##### Summary-Introduction.

E. BÖHM-VITENSE

*Institute of Theoretical Physics - Kiel*

Since there is the excellent summary on stellar convection given by J.-C. PECKER at the Liège symposium on stellar evolution in 1959, where he gives a complete account of the existing literature, I shall in this summary not try to do justice to the history of stellar convection research nor shall I try to mention all the existing papers even if they are quite important. I shall rather attempt to give an objective picture of the present state of affairs in order to be able to discuss our problems with the aerodynamicists. I shall cite only those papers that have been explicitly used for the results presented here. Additional references to literature may be found in the article by J.-C. PECKER.

##### Introduction.

The origin for turbulence in the atmosphere of stars with effective temperature  $T_{\text{eff}} \leq 8000^\circ$ —spectral types later than about A6—may be found in the convectively unstable zone which occurs a few hundred km below the surface of the stars due to the ionization of hydrogen (UNSÖLD, 1930). For stars with higher temperature this instability zone is too flat for convection with any measurable velocities to occur. But in connection with rapid rotation of the stars it may still cause some turbulent motion, as KIPPENHAHN has pointed out (KIPPENHAHN, 1950).

#### A) Observations.

##### 1. - Solar photosphere.

The velocity fields caused by the hydrogen convection zone can be best observed on the solar surface, which shows the well-known phenomenon of the solar granulation.

The main items that observers tried to investigate are the following:

- I. Size and shape of granules.
- II. The lifetime of granules.
- III. Size of the temperature fluctuations between granular and intergranular regions.
- IV. Velocity fluctuations between granular and intergranular regions.
- V. Correlation between temperature and velocity fluctuations.
- VI. Magnetic fields of granules.

In spite of the excellent observational work on which we heard reports yesterday, we can hardly provide definite answers to any of these problems.

The main difficulty for observations is the bad seeing, which means the small scale turbulence in the atmosphere of the earth that disturbs the wave front of the light and in this way prohibits excellent image quality. Even under best seeing conditions the resolution for observations from the ground is usually limited to about  $1''$  of arc, which corresponds to about 700 km distance on the solar surface.

Many attempts have been made to escape bad seeing. RÖSCH took pictures at the Pic du Midi Observatory; we saw one of his films yesterday. BLACKWELL, DEWHIRST, and DOLLEUSS took pictures from a manned balloon going up to 6 km height. SCHWARZSCHILD even took pictures from an unmanned balloon, reaching a height of 27 km. We also saw one of his films. Taking pictures from balloons has the advantage of escaping bad seeing conditions to a high degree, but it has the disadvantage of limited size of telescopes which can be borne by a balloon. The telescopes that have been used so far have an aperture of 12 in. = 30 cm, which gives a *theoretical resolving power of  $0''.4$  of arc*, which obviously is a lower limit to the actual resolving power.

We shall now proceed to discuss the observational results concerning items I to VI.

1.1 *Size and shape of granules.* — SCHWARZSCHILD (1959) found diameters of granules reaching from 300 to 1800 km, in agreement with Rösch's pictures, corresponding to about  $0''.4$  to  $2''.5$ , with a mean size of about 700 km. The granules are separated by dark, narrow lanes. The structure of the granules is irregular but very often polygonal. These findings are confirmed by the other observers. SCHWARZSCHILD does not think that the granules could be Bénard cells, because

- 1) The polygonal structure of the granules is irregular.
- 2) The granules have varying diameters.

- 3) The solar granulation is a distinctly non-stationary phenomenon, as we saw yesterday.

SCHWARZSCHILD compares the solar granulation to a type of fluid motion which is called «non-stationary convection» (SIEDENTOPF, 1948; PRANDTL, 1942). It is a type between the regular Bénard cells and a completely irregular turbulent convection, which occurs for Rayleigh numbers of the order  $10^5$  ( $R = (\Delta \text{ grad } T/T) \cdot (l^3 \cdot g/\nu \cdot \kappa)$ , with  $\nu$  — kinematic viscosity and  $\kappa$  — conductivity, being radiative conductivity in stellar atmospheres). For the upper solar convection zone SCHWARZSCHILD calculates  $R = 10^{10}$ .

There remains the question whether the observed sizes of granules are real or whether they are counterfeit by the limited resolving power of the instrument, so that the observed granules are really conglomerates of small granules which cannot be observed.

SCHWARZSCHILD tries to answer this question by calculating the autocorrelation functions for the brightness fluctuations on his images of the solar surface:

$$Q(y) = \frac{1}{N-m} \sum_{n=1}^{n=N-m} \Delta I_n \cdot \Delta I_{n+m}, \quad \text{with} \quad y = m \cdot 138 \text{ km}.$$

He then calculates the same function for what he calls a point model, which means he assumed that all granules are really very small bright spots distributed randomly and that the instrumental profile is given by the diffraction pattern of the 12 in. aperture. This theoretical  $Q(y)$  does not agree with the observed one. SCHWARZSCHILD also calculates  $Q(y)$  with the assumption that the granules are bright lines distributed randomly and finds much better agreement with the observations. From this he concludes that he really resolves the individual granules.

To me it seems that the possibility has not been excluded that the granules are really point-like, with a tendency to congregate. In this connection I should like to draw attention to the experimental investigation of FELLGETT (1959). He copied plate grains in a special way so as to show congregations of grains and then took out of focus photographs of these plate grains. It is amazing how much this blurred picture resembles the structure of a photograph of solar granulation, although it must be realized that the image quality of the best solar photographs is better than the one of this picture. But the main result is that by blurring the original picture the original plate grain is completely lost and, instead, there appears a network which was not visible on the original.

1'2. *The lifetime of granules.* — RÖSCH and SPIEGEL yesterday discussed this question. They obtained lifetimes of 6 to 8 minutes. It seems somewhat dif-

difficult to define a lifetime of granules because they change their appearance rather quickly but do exist a rather long time. On the other hand Leighton's investigations of line shifts and intensities indicated a lifetime of about 20 minutes but with an oscillation period of 5 minutes for the velocities. Since he obtains a correlation of brightness and velocities this period should also be present in the brightness of granules. Rösch's and Spiegel's pictures certainly did not show these 5 minute oscillations. All these different observations have been made with so much care and skill that I personally hate to doubt any one of them. So, I am faced with the very difficult problem of bringing these observations together into a consistent picture which we can place before the aerodynamicists. I must admit that I find it extremely hard to fit in these oscillatory motions with the other observations.

If we have to accept the oscillating motions as being real, the only way could be that these oscillations occur in such high layers that they do not show up at all in the continuous spectrum. As I pointed out earlier those parts of the line profile of the CaI line 6130 which are used to determine the Doppler shifts can be expected to be formed in layers with optical depths  $\tau_{\text{continuum}} = 0.05$  to  $0.01$  (corresponding to about 150 to 100 km below the sun's surface) or possibly even higher, while the continuous spectrum originates in  $\tau = 1$ . But then due to the correlation of brightness and velocity, which was observed by LEIGHTON, we should expect a correlation between line intensity and velocity which I do not think has been observed so far. Moreover, as WADDELL already pointed out some days ago, there has been derived a circulating motion just by investigating line profiles originating in high layers. We shall come back to this later.

In any case, for the granules seen in the continuum I am inclined to accept the lifetime of 6 to 8 minutes.

1'3. *Size of the temperature fluctuations between granular and intergranular regions.* — THIESSEN'S (1955) visual observations with the 60 cm refractor in Bergedorf (resolving power  $0''.17$  of arc) indicated mean brightness fluctuations of 35%, corresponding to mean temperature differences of  $370^\circ$  ( $\pm 185^\circ$ ).

SCHWARZSCHILD on one side and BLACKWELL, DEWHIRST, and DOLLFUSS on the other side measured the brightness fluctuations on their new plates and both found  $\sqrt{\Delta I^2} = \pm 0.046$ . The English-French group then pointed out that, due to the resolving power of the instrument of  $0''.4$ , features of  $1''$  diameter lose much of their contrast; in fact, they measure the contrast transmission function for their instrument as a function of size of observed features with an object of known contrast. They found that the measured brightness fluctuations correspond to real temperature differences of at least  $520^\circ$  ( $\pm 260^\circ$ ). This appears to be a lower limit, because for the correction the seeing conditions were not taken into account, and the mean granular and intergranular



diameters were assumed to be  $1''.4$ , while the intergranular regions might be much narrower. For smaller features the correction to the measured contrast must be much greater.

This is the only direct method to determine temperature fluctuations independently of all model calculations for the solar atmosphere.

There are other indirect methods to derive temperature fluctuations in the solar photosphere, which indicate values in agreement with the one determined from the brightness fluctuations. First one can investigate the center-to-limb variations of the line intensities. This has been done so far under the assumption of L.T.E., which to me still seems to be a fairly adequate assumption. For each line there is a certain well-known temperature-dependence of intensity. In addition the regions with higher temperature contribute more to the observed mean intensity than do the colder regions. Therefore the observed line intensity is different from the one which we would expect to observe in a homogeneous photosphere.

K. H. BÖHM was the first to derive values for  $\Delta T$  by studying the center-to-limb variation of FeI line wings. He found that he got relatively good agreement with observations using a three-stream model with  $\Delta T \sim \pm 1000^\circ$  in  $\bar{\tau}=1$  for equal geometrical depths  $t$ ; corresponding to  $\Delta T = \pm 500^\circ$  in equal optical depths  $\tau$ . (Observations of contrast always refer to equal optical depths, because radiation always escapes from  $\tau = \cos \vartheta$ .) Since the absorption coefficient  $\kappa$  is larger for higher  $T$ , the apparent  $\Delta T$  for equal optical depths,  $\tau = \int \kappa dt$ , are smaller than  $\Delta T$  in equal geometric depths. The situations for a three-stream model is sketched in the following draft.

$T^A = 0.4T_0^A$	$T^A = T_0^A$	$T^A = 1.6T_0^A$
$\Delta T = -1200^\circ$	$\Delta T = 0$	$\Delta T = +730^\circ$
		$6400^\circ$
		$\tau = 1$
$4800^\circ$	$6000^\circ$	$6735^\circ$
$5400^\circ$	$\tau = 1$	
$\tau = 1$		

H. H. VOIGT investigated in 1956 the center-to-limb variation of the infrared oxygen triplet being formed in rather deep photospheric layers where one would certainly not expect serious deviations from L.T.E. He found best agreement with observations assuming a three-stream model with  $\Delta T = \pm 1000^\circ$  in deep layers but decreasing with height,  $\Delta T$  being 0 in  $\tau_{\lambda=7775} = 0.03$ .

SCHRÖTER (1957) finds agreement with measured red shifts and center-

to-limb variations of weak FeI lines, assuming a two-stream model with  $\Delta T = \pm 450^\circ$  (in equal  $t$ ) for  $\bar{\tau} = 0.6$ , decreasing in both directions upwards and downwards.

This is also in rough agreement with the observations of the Balmer lines. Using the new theory of hydrogen line broadening evaluated by GRIEM, KOLB and SHEN (1959) and taking into account the resonance broadening, TRAVING and CAYREL (1960) found that temperature fluctuations of  $\leq \pm 300^\circ$  in equal optical depths are still permitted by observations.

All these indirect determinations of temperature fluctuations are of course subject to great uncertainties, but they show that all investigations of line profiles suggest temperature fluctuations  $\Delta T$  of the same magnitude as were derived by the direct photographs of the sun's surface, which means  $\Delta T \geq \pm 260^\circ$  in equal optical depths.

1.4. *Velocity fluctuations between granular and intergranular regions.* — Again we shall first deal with a direct method not making use of any assumptions about the source function  $S(\tau)$ .

In 1950 RICHARDSON and SCHWARZSCHILD obtained a solar spectrogram on Mr. Wilson—with the slit put across the sun's image—which clearly showed a wiggling of the Fraunhofer lines attributed to different velocities on different points of the sun's disc. Miss MÜLLER will show us such a spectrogram this morning. RICHARDSON and SCHWARZSCHILD measured  $\sqrt{2} \Delta v = 0.37$  km/s. At the same time they found brightness fluctuations of 6.4%. THIESSEN (1955) compared this value with the 35% brightness fluctuations found by him and concluded that Richardson and Schwarzschild's values were very much reduced by scattered light. The scattered light must then also reduce the velocity fluctuations. He calculated that the measured value of  $\sqrt{2} \Delta v = 0.37$  km/s would mean true velocity fluctuations of 1.85 km/s.

Again there are indirect methods to determine mass motions. We can, for instance, measure the line profiles—integrated over several granules as one usually does when taking a spectrum of the sun. If we assume the non-thermal velocities to have a gaussian distribution, then the width of faint lines will be determined by  $\Delta \lambda_D = \lambda \cdot (\xi/c)$  where  $\xi = \sqrt{\xi_{\text{therm}}^2 + \xi_{\text{turb}}^2}$ .  $\xi$  is the velocity component in the line of sight. For stronger lines one has to correct for saturation effects.

By observing the center-to-limb variation of line profiles one can even determine the depth-dependence of the velocities. At the limb only the high layers contribute to the line profile, while in the center deeper layers also are seen.

The investigations of ALLEN (1949), REICHEL (1953), VOIGT (1956), SUEMOTO (1957), and WADDELL (1958) agree that in the center of the sun the mean line-of-sight velocity, which is there the radial component, is (1.7 : 1.8) km/s:

while at the limb, the line-of-sight velocity, which is there mainly the horizontal component, is around 2.8 km/s. (The lines are observed to be broader at the limb.)

VOIGT, taking into account also the temperature inhomogeneities, found agreement with observations, only by assuming an outward decreasing radial component  $\sim 3$  km/s at  $\tau = 1$  and an outward increasing tangential component ( $v \sim$  km/s in  $\tau = 0.02$ , called by him microturbulence). The decreasing radial component was confirmed by SUMOTO's paper, where he points out that weaker lines of the same multiplet have a larger Doppler width than to the stronger lines. The weaker lines are formed in deeper layers. On the other hand all lines are broadened toward the limb, indicating an increasing horizontal component of the velocities.

UNNO (1959), studying various points in line profiles in the center of the disc being formed in various depths, also obtained an outward decreasing radial velocity component.



Fig. 1.

Altogether we obtain the picture of a circulating motion.

There is only a slight discrepancy with a result of MIYAMOTO (1954) who obtained  $v_{\text{rad}} = 4.6$  km/s for those layers, where the cores of strong Fraunhofer lines are formed, which may however already be in the low chromosphere.

There is still the curve-of-growth method to determine non-thermal velocities. This, I think, is quite valuable for other stars, but for the sun the direct measurement of center-to-limb variation of line-profiles gives more accurate results. There seems to be a general tendency, however, toward somewhat smaller values for  $\xi_{\text{turb}}$  ( $\sim 1.5$  km/s) derived from curve-of-growth analysis than from line-profile method. If this is true there may be two reasons.

Curve of growth shows only small scale motions, while the line-profile includes also large scale motions. So the presence of large scale motions probably shows up (the same velocities over regions  $> \tau = 1$  ( $> 400$  km)). On the other hand, for curve-of-growth analysis strong lines originating in high layers have the highest statistical weight, while for measuring line-profiles one usually uses weaker lines originating in deeper layers. So a stratification effect may also be included in the different results.

1.5. *Correlation between temperature and velocity fluctuations.* — A correlation of  $\Delta T$  and  $\Delta v$  has always been expected, but to my knowledge it seemed first to be proved to exist by STUART and RUSH in 1954. Their investigations were based on the spectrogram of Richardson and Schwarzschild. They found a correlation between the small scale fluctuations in brightness and velocities in the way that the brighter regions have outward velocities (correlation coefficient  $r = -0.50$  to  $-0.68$ ).

The  $\Delta v$  were determined as deviations from a running mean. This gave  $\sqrt{2 \overline{\Delta v^2}} = 0.17$  km/s.

The discussion of FELLGETT (1959), however, shows that this value of  $r$  is not so significant as has been supposed thus far. The subtraction of a running mean is dangerous. Without using running means one obtains  $r \approx -0.3$ , and this obviously does not mean very much, because FELLGETT in one case even obtained  $r = -0.41$  when comparing velocity fluctuations on one spectrogram with the brightness fluctuations on another one taken 20 minutes later. But FELLGETT also states that exceedingly bright granules are always correlated with negative velocities (rising matter).

It may not be surprising that the observed correlation between  $\Delta T$  and  $\Delta v$  is rather weak. First there are the difficulties of accurate measurements because of scattered light, and second we have to keep in mind that brightness and velocity variations refer to quite different depths, as was pointed out earlier.

**1'6. Magnetic fields of granules.** — Magnetic fields of granules have not been observed so far, although they have been looked for. It seems that magnetic fields larger than 50 gauss correlated with granules do not exist.

## 2. — Summary of granule observations.

Finally we may summarize the observational results concerning solar granules. Mean diameters are  $\leq 700$  km. The observed brightness fluctuations on the solar surface, observed as the phenomenon of granules, correspond to temperature fluctuations  $\Delta T \geq 520^\circ$  in equal optical depths.  $\Delta T$  is probably decreasing with height. The velocity fluctuations are about  $(2.5 \div 3.0)$  km/s, changing their direction from vertical motions in deeper layers ( $\tau = 1$ ) to horizontal motions in high photospheric layers. Very bright granules certainly rise, less bright ones probably do. Magnetic fields connected with granules, if present, must be smaller than 50 gauss.

## 3. — Velocities in sunspots.

BRAY and LOUGHEAD (1959) have published photographs of granules in sunspots which possibly have somewhat smaller diameters than the ones in the photosphere. HOWARD (1958) found from curve-of-growth analysis velocities  $v = (2.9 \div 3.7)$  km/s. These are larger than the ones derived for the surrounding photosphere.



There are also the line shifts in the sunspots penumbra corresponding to a nearly horizontally outwards streaming gas with maximum velocity of about 3 km/s for a large spot—usually called the Evershed effect.

#### 4. – Velocities in the chromosphere.

The observations of velocities in the solar chromosphere have been summarized by DE JAGER in his article in the *Handbuch der Physik* (1959). Velocities increase with height up to about 15 km/s in about 3 000 km above the solar limb.

#### 5. – Velocities arising from convection in other stars.

The observational data are reviewed in the article of WRIGHT in the *IAU Transactions* (1955). Miss UNDERHILL has included the latest observations in her summary talk. The measured velocities increase with increasing effective temperatures of the stars and with decreasing surface gravity. You still have the table of Miss UNDERHILL.

### B) Theory of the Hydrogen Convection Zone.

Convection occurs when

$$\nabla = \frac{d \log T}{d \log P_g} > \frac{d \log T}{d \log P_{g \text{ adiabatic}}} = \nabla_{ad}.$$

In the high photospheric layers of a star this is not fulfilled; they are in radiative equilibrium, meaning that the whole energy transport is performed by radiation. In such an atmosphere the temperature distribution is given approximately by

$$(1) \quad T^4 = \frac{3}{4} T_{\text{eff}}^4 \left( \bar{\tau} + \frac{2}{3} \right) \quad \text{with} \quad \sigma T_{\text{eff}}^4 = \pi F, \quad \pi F = \text{net flux},$$

while the distribution of the gas pressure  $P_g$  obeys the hydrostatic equation

$$(2) \quad \frac{dP_g}{d\bar{\tau}} = \frac{g}{\kappa/g\tau}, \quad g = \text{gravitational acceleration}$$

For the sun  $T_{\text{eff}} = 5800^\circ$ ,  $g = 2.82 \cdot 10^4$ .

From these equations one obtains

$$(3) \quad \nabla_{\text{rad}} = \frac{d \log T}{d \log P_g} \text{ (radiative equilibrium)} = \frac{3}{16} \frac{\bar{\kappa} P_g}{g} \left( \frac{T_{\text{eff}}}{T} \right)^4.$$

The gradient has to be proportional to the flux  $\sigma T_{\text{eff}}^4$ . If this gradient becomes larger than the corresponding one for adiabatic stratification the layer will be convectively unstable. This may happen for two reasons: 1)  $\nabla_{\text{ad}}$  becomes very small, or 2)  $\nabla_{\text{rad}}$  becomes very large.  $\nabla_{\text{ad}} = (\gamma - 1)/\gamma$ , if  $\gamma$  were constant, where  $\gamma = c_p/c_v$ .  $\nabla_{\text{ad}}$  becomes very small if  $\gamma$  comes close to unity. This happens if  $c_p$  is very large, which in stellar atmospheres occurs in those layers where the most abundant element hydrogen is ionized, which means in layers with  $T \approx 10\,000^\circ$ .  $\nabla_{\text{rad}}$  may become quite large when  $\bar{\kappa}$  becomes very large. This happens in stellar atmospheres for  $T \geq 7\,000^\circ$ .

In those stellar atmospheres in which we are interested, the continuous absorption is mainly due to  $H^-$  absorption and hydrogen absorption in the Paschen continuum, that means absorption from the third quantum level of hydrogen. Around  $T \sim 7\,000^\circ$ , the excitation degree of the third quantum level becomes high enough and increases rapidly, so that hydrogen absorption exceeds  $H^-$  absorption and increases rapidly with  $T$ , until  $T$  becomes so large that hardly any neutral hydrogen is left over. For such high temperatures the absorption coefficient will then decrease.

So  $\nabla_{\text{ad}}$  decreases and  $\nabla_{\text{rad}}$  becomes very large for about the same  $T$ . Both effects together cause quite an active convection.

Since the upper boundary of this unstable layer occurs in  $\tau = 0.8$ , the convection zone contributes appreciably to the observed radiation. Therefore astrophysicists are interested especially in the temperature stratification of these layers. The temperature stratification depends on the amount of energy which is transported by radiation. As I said, the gradient  $\nabla$  is proportional to the radiative flux  $\pi F_{\text{rad}}$ . If  $\pi F_{\text{rad}} < \sigma T_{\text{eff}}^4$ , we have to put  $\pi F_{\text{rad}}$  into eq. (3) instead of  $\sigma T_{\text{eff}}^4$  and obtain

$$(4) \quad \nabla = \frac{3}{16} \frac{\bar{\kappa} P_g \pi F_{\text{rad}}}{g \sigma T^4}.$$

If we know  $\pi F_{\text{rad}}$  we can calculate  $\nabla$  and the temperature-pressure stratification

$$(5) \quad \Delta \log T = \int_{P_{g_0}}^{P_g} \nabla d \log P_g.$$

In equilibrium the amount of energy transported through the atmosphere must

be independent of depths:  $dF/dt = 0$  which means

$$(6) \quad \pi F_{\text{rad}} + \pi F_{\text{conv}} = \pi F = \sigma T_{\text{eff}}^4.$$

(Energy transport by conduction may be neglected.) All we have to know is  $\pi F_{\text{conv}}$ . So the primary interest of astrophysicists is the amount of convective energy transport, which can be expressed as

$$(7) \quad \pi F_k = c_p \rho T \frac{\overline{\Delta T}}{T'} \cdot \bar{v},$$

where the mean should be taken over the horizontal plane in question. The problem then is to calculate  $\overline{\Delta T}$  and  $\bar{v}$ .

To my knowledge this has only been done in the approximation of a so-called mixing length theory, which in this connection means something different from the mixing length theory applied to turbulent shear flow and should perhaps be better called «characteristic-scale» approximation. The Rayleigh numbers in stellar atmospheres are very large due to the vast dimensions, so we may expect the convection to be turbulent. In the mixing length approximation it is assumed that only turbulence elements of size  $l$  exist and that they will travel this same length  $l$  and then disappear as turbulence elements.

This kind of theory was first applied to stellar atmospheres by SIEDENTOPF (1935) and BIERMANN (1942).

In the convection zone at a given point  $P$  we have the following situation:

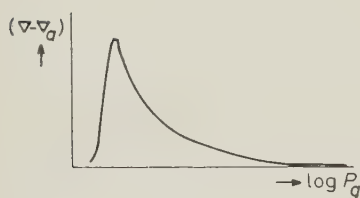


Fig. 2.

The mean logarithmic temperature gradient is  $\nabla$ . The adiabatic gradient for the given  $T$  and  $P_g$  is  $\nabla_a$ , which is much smaller than  $\nabla$ . A bubble that would start rising in  $P$  will rise with a somewhat steeper gradient  $\nabla'$  than  $\nabla_a$ , due to energy exchange with the surrounding matter.

The temperature difference  $\Delta T$  is then proportional to  $\nabla' - \nabla$ .

With the above assumptions of mixing length theory (VITENSE, 1953) we obtain

$$(8) \quad \frac{\overline{\Delta T}}{T} \sim (\nabla - \nabla') \frac{l}{2H} \quad \text{giving} \quad \pi F_k = c_p \rho T \bar{v} (\nabla - \nabla') \frac{l}{2H},$$

where  $H = RT/\mu g$ .

In deriving this equation we have assumed that  $\nabla - \nabla'$  is constant over a distance  $l$  and that  $\overline{\Delta T} = \Delta T (x = l/2)$ .

The velocity is derived by the integral

$$(9) \quad \frac{m}{2} v^2 = \int_0^x K(x) dx,$$

with  $K = -g \cdot \Delta \varrho \cdot \text{volume of the bubble}$ .  $\Delta \varrho$  is connected with  $\Delta T$  by  $\Delta \varrho / \varrho = (\Delta T / T) \cdot Q$ , where  $Q = 1 - \partial \log \mu / \partial \log T$  takes care of the change in mean atomic weight due to changes in the degree of ionization.  $x$  is the co-ordinate, corresponding to geometrical depth.

The difference  $\nabla_{\text{ad}} - \nabla'$ , is determined by the energy loss of the bubble on its way, which means by radiative energy exchange.

With these equations we can calculate  $\Delta T$  and  $\bar{v}$  and the stratification in the convective layer, if we find a proper size for the characteristic scale  $l$ , which we did not yet determine.

We did introduce the characteristic length  $l$  in order to find an equilibrium value for  $\bar{v}$  and  $\Delta T$ . The normal instability calculations yield a circulating motion with increasing velocity as long as we regard only the linear terms in the hydrostatic equations. We should find an equilibrium value for  $v$  if we take into account all the energy dissipating terms. A first step in this direction was made by MALKUS and VERONIS (1958) who considered a case with a relatively small Rayleigh number. I heard that SCHWARZSCHILD, LEDOUX and SPIEGEL have tried to include turbulent viscosity. We shall probably hear about these attempts later.

We started from another viewpoint, assuming that the circulating motion does not really exist as a full circle, but that the velocity increase of rising bubbles is terminated because they are disturbed so much on their way that they do not exist any more as a unique feature. The question then is how far can they travel without losing their identity. This length we shall take as the characteristic length  $l$ . According to the assumptions of a mixing length theory this same length will then also determine the linear extension of the bubbles. For the numerical calculations  $l = H$  was assumed for the following reasons: Primarily we want to calculate the convective energy transport. Small bubbles will lose their surplus energy rather quickly due to radiative energy exchange. The largest bubbles will lose the least amount of energy and therefore transport most of it. On the other hand, with the assumption of rising bubbles we cannot make the bubbles very large, for otherwise they could not exist as a unique feature. Also the bubbles will have changed their internal structure appreciably after having traveled one scale height and will therefore essentially lose their identity. These considerations give an upper limit  $l \leq H \cdot a$ , where  $a$  is of the order of unity. But the bubble cannot, of course, be assumed to be larger than the whole unstable layer. If the unstable



layer is less thick than one scale height we have to assume the characteristic length to be of the order of the height of the unstable layer.

The results that have been derived with these assumptions are given in a paper by BÖHM-VITENSE (1958).

The convective energy transport can be neglected down to optical depths  $\bar{\tau} = 2$ . The extension of the convection zone in the sun is 60 000 km; it is larger for lower effective temperatures (90 000 km for  $T_{\text{eff}} = 5\,000^\circ$ ) and smaller for higher effective temperatures (4 000 km for  $7\,000^\circ$ ). For even higher temperatures the height of the convective layer comes out to be smaller than the scale height. For these stars convection must stop rather abruptly because we have to assume the most unstable bubbles to be smaller than the scale height. Smaller bubbles will have a much greater energy exchange, so the energy transport is reduced. This makes the convection zone still narrower. (The gradient  $\nabla$  becomes steeper, so higher temperatures are already reached for relatively low pressures, and the hydrogen is already ionized in higher layers.) The size of the bubbles has to be reduced again, and so on. We obtain a very narrow unstable zone in radiative equilibrium. The velocities that can be expected are of the order of 1 cm/s. For main sequence stars this occurs for  $T_{\text{eff}} \approx 8\,000^\circ$ . For giants and supergiants it occurs for much lower temperatures ( $4\,400^\circ$  for very bright supergiants).

The calculated velocities are of the same order of magnitude as the observed ones (perhaps, somewhat lower). For the sun one calculates, for example, close to the upper boundary of the convection zone  $\bar{v} = 1.7$  km/s. (A factor  $\frac{1}{2}$  was introduced in  $\bar{v}$  in order to take into account the turbulent friction. Probably this should not be done in a mixing length theory.)

The calculated velocities show the same trend as the observed ones, becoming larger with higher  $T_{\text{eff}}$  and lower surface gravity, but suddenly decreasing when convective energy transport becomes negligible. This last result does not agree with observations. For hot stars we must therefore look for

another mechanism which can give high turbulent velocities. Perhaps we should come back to this point in the discussion.

The assumption  $l = H$  was first introduced into theory of stellar convection zones by Biermann. During the last several years it has been subject to much criticism. What other length could be introduced as the characteristic length? In any case we should take the size of the most unstable bubbles

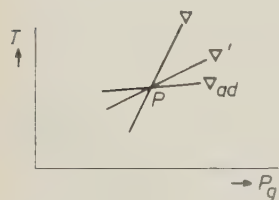


Fig. 3.

which transport the main amount of energy. There do exist a number of instability investigations.

The distribution of instability  $(\nabla - \nabla_{\text{ad}})$  is generally given by the above graph.

The layer, which is really very unstable, extends only a few hundred km, usually the same order of magnitude as the scale height. (This result does not depend sensitively on the assumption about mixing length.)

BÖHM (1958) calculated the size of the most unstable wave numbers in the Rayleigh way for convection zones consisting of two layers with very different degrees of instability. In the very unstable zone the most unstable wave numbers are those which correspond to the height of the zone with high instability, regardless of the extension of the less unstable zone. (Density variations were neglected.) If this length ought to be taken as the characteristic length it would again be equal to the scale height within a factor of 2 or 3.

SKUMANICH (1955) made investigations of the instability for an atmosphere of decreasing density but unique degree of instability. His result was an increasing instability for smaller wave numbers, but BÖHM and RICHTER (1959) repeated the same calculations taking into account the radiative energy exchange which will of course reduce the instability of small wave numbers. They found that for conditions in the sun one should expect the largest instability for wave lengths  $> 300$  km/s (perhaps larger by a factor of 2).

So all the various heights that might be suggested by these investigations lead to the same order of magnitude for  $l$  as was assumed at least for the high layers (\*).

The observations show a size of granules of 700 km, probably corresponding to the most unstable wave number in the upper part of the convection zone. On the other hand,  $700 \text{ km} = \frac{3}{2} H$  for the depth from which those bubbles should rise, which we see on the surface.

So for the high layers our numerical results, concerning the stratification in the convective zone, may be expected to be right within a factor of 2 or 3 (with regard to  $\pi F_{\text{conv}}$ ) if we assume  $l = H$ . For the deeper layers this assumption may not always be right because  $H$  increases with increasing  $T$  and possibly  $l$  should be fixed and connected with the extension of the unstable layer. But in deep convective layers we shall always find  $\nabla \approx \nabla_{\text{ad}}$  regardless of the assumptions about  $l$ .

There has been criticism against using this kind of mixing length theory at all. Of course it can only be regarded as a first order approximation. What

(\*) The order of magnitude agreement between the size of the most unstable zone and the size of the most unstable wave number is due to the fact that  $\nabla$  becomes about equal to  $\nabla_{\text{ad}}$ , meaning that instability becomes small when radiative energy exchange over a distance  $l$  is negligible. The most unstable wave length also corresponds to the smallest extension for which radiative energy exchange is negligible. Since  $H = RT/\mu g$  is always of the same order of magnitude as the geometrical depths to the point in question, all the possible characteristic scale heights appear to be necessarily of the same order of magnitude.

ought to be done is to solve the exact hydrodynamic equations for a stationary state, *e.g.*, with a first approximation convection zone (as, for instance, the ones described above); then one has to calculate the energy transport and  $\pi F_{\text{rad}}$ . Having this, one could calculate a better stratification for the convection zone, solve again the hydrostatic equations, and so on. To my knowledge, nobody has as yet succeeded in doing so, but we shall probably hear from MALKUS about investigations that may be used for a step in this direction. The basic difference of the stellar case in comparison with laboratory experiments seem to be that we do not know the lower boundary conditions for the convection zone. These themselves depend on the solution for  $\pi F_{\text{conv}}$ .

There has also been criticism against regarding the observed granulation as rising and falling gas. In 1953 and 1954, SCHATZMAN and THOMAS proposed the granules to be the appearance of acoustic waves (see also WHITNEY, 1958). According to the investigations of LIDTHILL (1955), there will be generated acoustic waves in a turbulent velocity field. Part of these will certainly travel upwards and will be amplified due to the rapidly decreasing density in high photospheric and chromospheric layers (SCHIRMER, 1950). Probably they will finally become shock-waves, which as far as we know are the main agency for the rising temperature in the chromosphere and corona (SCHWARZSCHILD, 1948; BIERMANN, 1948). However I do not think that these acoustic waves have any important influence on the appearance of the granulation. I do not see any reason against assuming the granules to be rising and falling matter even in the convectively stable radiative zone, because they can easily overshoot. This means the rising gas will reach the upper boundary of the convective layer with a surplus temperature, and therefore will still be accelerated into the radiative zone. Theoretical investigations (UNNO, 1957; BÖHM and RICHTER, 1960) show that we have to expect a circulation in the radiative zone with nearly the same absolute velocities as in the upper part of the convection zone, exactly in the way that is observed by ALLEN, WADDELL, and SUEMOTO.

## REFERENCES

- ALLEN, C. W., 1949, *M. N.*, **109**, 343.  
 BIERMANN, L., 1937, *Astr. Nachr.*, **264**, 359.  
 BIERMANN, L., 1942, *Z. f. Astrophys.*, **21**, 323.  
 BIERMANN, L., 1948, *Z. f. Astrophys.*, **25**, 135, 161.  
 BLACKWELL, D. E., D. W. DEWHIRST and A. DOLLFUS, 1959, *M. N.*, **119**, 98.  
 BÖHM, K. H., 1954, *Z. f. Astrophys.*, **35**, 179.  
 BÖHM, K. H., 1958, *Z. f. Astrophys.*, **46**, 245.  
 BÖHM, K. H. and E. RICHTER, 1959, *Z. f. Astrophys.*, **48**, 231.  
 BÖHM, K. H. and E. RICHTER, 1960, *Z. f. Astrophys.*, **50**, 79.

- BÖHM-VITENSE, E., 1958, *Z. f. Astrophys.*, **46**, 108.
- BRAY, R. J. and R. E. LOUGHEAD, 1959, *Austral. J. of Phys.*, **12**, 320.
- DE JAGER, C., 1959, *Handbuch der Phys.*, **52**, 80.
- FELLGETT, P., 1959, *M. N.*, **119**, 475.
- FRENKIEL, F. N. and M. SCHWARZSCHILD, 1952, *Ap. J.*, **116**, 422.
- FRENKIEL, F. N. and M. SCHWARZSCHILD, 1955, *Ap. J.*, **121**, 216.
- GRIEM, H. K., A. C. KOLB and K. Y. SIEN, 1959, *Phys. Rev.*, **116**, 4.
- HOWARD, R., 1958, *Ap. J.*, **127**, 108.
- KIPPENHAHN, R., 1959, *Z. f. Astrophys.*, **48**, 172.
- LIGHTHILL, M. J., 1955, *IAU Symp.*, **2**, 121.
- MALKUS, W. V. R. and G. VERONIS, 1958, *Jour. Fluid Mechanics*, **4**, 225.
- MIYAMOTO, S., 1954, *Publ. Astron. Soc. Japan*, **6**, 150.
- PECKER, J. C., 1960, *Modèles d'étoiles et évolution stellaire*, Colloque internationale du 6 au 8 Juillet 1959. Printed in: *Les Congrès et Colloques de l'Université de Liège*, vol. **16**.
- PLASKETT, H. H., 1954, *M. N.*, **114**, 251.
- REICHEL, M., 1953, *Z. f. Astrophys.*, **33**, 79.
- RICHARDSON, R. S. and M. SCHWARZSCHILD, 1950, *Ap. J.*, **111**, 351.
- RÖSCH, J., 1959, *Ann. d'Astrophys.*, **22**, 571, 584.
- SCHATZMAN, E., 1953, *Bull. Ac. Roy. Belg., Cl. Sc., 5e Série*, **39**, 960.
- SCHATZMAN, E., 1954, *Bull. Ac. Roy. Belg., Cl. Sc., 5e Série*, **40**, 139.
- SCHIRMER, H., 1950, *Z. f. Astrophys.*, **27**, 132.
- SCHRÖTER, E. H., 1957, *Z. f. Astrophys.*, **41**, 141.
- SCHWARZSCHILD, M., 1948, *Ap. J.*, **107**, 1.
- SIEDENTOPF, H., 1935, *Astron. Nachrichten*, **255**, 157.
- SKUMANICH, A., 1955, *Ap. J.*, **121**, 408.
- STUART, E. E. and J. H. RUSH, 1954, *Ap. J.*, **120**, 245.
- SUEMOTO, Z., 1957, *M. N.*, **117**, 2.
- THIESSEN, G., 1955, *Z. f. Astrophys.*, **35**, 237.
- THOMAS, R. N., 1954, *Bull. Ac. Roy. Belg., Cl. Sc., 5e Série*, **40**, 621.
- TRAVING, G. and R. CAYREL, 1960, *Z. f. Astrophysik*, (in press).
- UNNO, W., 1957, *Ap. J.*, **126**, 259.
- UNNO, W., 1959, *Ap. J.*, **129**, 357, 388.
- UNSÖLD, A., 1930, *Z. f. Astrophys.*, **1**, 138.
- UNSÖLD, A., 1955, *Physik der Sternatmosphären*, 2. Auflage, (Berlin).
- VITENSE, E., 1953, *Z. f. Astrophys.*, **32**, 135.
- VOIGT, H. H., 1956, *Z. f. Astrophys.*, **40**, 157.
- WADDELL, J. H., 1958, *Ap. J.*, **127**, 284.
- WHITNEY, CH., 1958, *Smithsonian Contributions*, **2**, no. 12.
- WRIGHT, K. O., 1955, *IAU Transactions*.



## PART IV.

### Considerations on Localized Velocity Fields in Stellar Atmospheres: Prototype — The Solar Atmosphere.

#### A. - Convection and Granulation.

#### Discussion.

*Chairman:* W. H. McCREA

— W. V. R. MALKUS:

Consider the variations of the gradient that would be computed just from radiation theory. There would be some subadiabatic region, an adiabatic region, and then again a subadiabatic region, in the absence of heat transport by motion. This adiabatic region, of course, would be called a convective zone and there would be penetrations into the regions beyond—both above and below. Now, the astrophysicist, as I understand it, has in the past often assumed that wherever one computed instability using radiative transport alone, he could then recompute the atmospheric structure assuming that in this region convection carried all of the excess heat flux, and that it really stayed at an adiabatic gradient. The convective region has really gotten bigger when one has made this assumption. Now, clearly, that extreme is never quite realized. We've seen here how one reduces it a little bit by assuming that one must have a finite difference between the adiabatic gradient and that achieved through the convection process. But one might anticipate that the actual gradient would be rather closer to this extreme than it was to the initial picture: that is, that the convection both lengthens the region in which convection occurs and greatly reduces the superadiabatic gradient. Now, how much of a departure from adiabatic actually exists apparently is important to the astrophysicist, because he wants to compute temperatures in the interior of stars, and he has to do it by some theoretical computation that carries him below the gradients observed at the surface. Even this small difference, I'm told by SCHWARSCHILD, can make a difference in the interior temperature of the star. I am no authority on how important that difference is. In fact, at first glance, thermodynamicists might wonder why one couldn't get perfectly satisfactory stars just by integrating in and whenever you got to a superadiabatic region, calling it adiabatic, assuming there is convection there, till you get off the adiabatic region again, and radiation can carry the

entire heat flux. But we're told this isn't true, and, in addition, one wants to know more about the dynamics of the motion in these regions. Perhaps one wants to know how much beyond these regions convection can penetrate due to inertial features. In fact, as penetration occurred into the stable region above, one might expect smaller scales of motion to disappear rapidly. This is important because it is all we see of the sun. We only see the region where convective elements are penetrating into the stable layer (and, at best, a little bit below that). It is, unfortunately, in just this penetration region here that we must look more carefully at the dynamics, and can't accept very simple explanations of a constant mixing length or a mixing length depending only on local scale height. The region in question extends roughly one scale height, and in that region the convection goes from highly correlated velocity and temperature fields which transfer lots of heat, to velocity and temperature fields that are just left over after the penetration and have no correlation having been turned back by the stabilizing layer.

Now, I don't pretend to be able to deal even roughly with the problem in this complicated situation but I wanted to describe briefly a much simpler situation in which one can explore penetrative convection. It is oversimplified but if one wants to explore the dynamics of penetration of a convective motion into a stable layer, one may get some insight through certain laboratory experiments. We can see how a system of this sort can have its convective region altered by the penetration process, we can perhaps test hypotheses regarding the nature of penetrative convection in such controlled laboratory experiments, and then with some confidence in these hypotheses, apply them to the sun. Rather than heating from below, the experiment I'd like to describe involves cooling from below. Take a layer of ordinary distilled water and put it on a block of ice, or have a lower surface which has a temperature of  $0^{\circ}\text{C}$ , then an upper surface which has some temperature—assume the simplest case,  $100^{\circ}\text{C}$ ; boiling at the top and freezing at the bottom. Now, in this case, the temperature gradient is roughly linear in the absence of motion. However, since the density reaches a maximum at  $4^{\circ}\text{C}$ , there is a reversal of density and this whole lower layer is potentially unstable. When the dimensions of that region are such that the Rayleigh number is comparable to  $10^3$ , convective motions start in such a layer, cooled from below. What can it do? Well, if convection starts near the base, it will soon hit the stable region; there will be a certain penetration—alterations of the field. The convection carries heat, as it must release potential energy: then the gradients at the boundary must sharpen to carry the additional heat. If they sharpen in this lower boundary region they must sharpen throughout the entire stable region, and the  $4^{\circ}\text{C}$  water will occupy a much larger portion of the flow. Then we have convection which has altered the dimensions of the region in which instability occurs, and increased the heat flux. One of the things one wants to see is how far the

motions press beyond the point of maximum density. The only controllable parameter is the spacing between the two surfaces. One would like to explore, as much as possible, the dynamics of this type of convection which can alter its own boundary conditions.

Another facet of this experiment is that the stable region is a stratosphere of sorts and can have wave-like motions in it driven by the convection at its base. I can only cite two achievements in this study so far. One was the stability problem. If one deals with a density profile that is parabolic, one has a Rayleigh problem with a single non-constant coefficient. We can solve this problem. It leads to eigenfunctions which are large in the unstable region as you might expect, and drop off in an exponential way in the stable region. The other result concerns the first experiments with very crude temperature measuring equipment. We observed the changes in gradients anticipated above—and the level to which the convection penetrates was at  $8^{\circ}\text{C}$  to  $8\frac{1}{2}^{\circ}\text{C}$ . This penetration is well beyond the point of maximum density.

I believe this type of problem offers some hope of understanding aspects of the aerodynamics of the penetration in that region where we may expect simple, heuristic theories like mixing-length arguments to cause us some difficulty.

— E. BÖHM-VITENSE:

I think that in astrophysics the question of the upper transition region is not quite as serious as was pointed out by MALKUS. I do agree that the calculations with mixing-length theory are wrong at this point, for one reason: In our theory we always assume that the values at the point in question are mean values over a region extending from half the mixing-length below and from half the mixing-length above the point in question. If we then calculate the convective energy transport as being proportional to the difference between the actual temperature gradient and the adiabatic one, we will, of course, get convective energy transport zero, at the transition point to the stable layer, which is, of course, not true because we have moving matter through this point. But on the other hand, if we just calculate from the observation the amount of convective energy transport which we have in this region—or we can take our model and start calculating the amount of energy transport—it comes out to be just about 5% of the whole energy transport. And this modifies the temperature gradient only very little. Therefore I don't think that the calculated stratification of this transition region is much influenced by the assumptions we have made.

— L. BIERMANN:

What is the Reynold's number associated with these motions, these convective motions, in this experiment? Is it large compared with  $10^3$ , or is it small? Or to put it otherwise, is the convection stationary or non-stationary?

— W. V. R. MALKUS:

There seem to be two types of convection in the experimental situation. It may help to describe them relative to an experimental plot of the dependence of heat flow on Rayleigh number. I plot the log of the Rayleigh number as abscissa, and as the ordinate the log of the Rayleigh number times the Nusselt number, which for the astrophysicists would be the ratio of the effective coefficient of heat transport over the actual coefficient of heat transport. If there were no motion, the plot would be a straight line, which would correspond to pure conduction. Now, in the experimental situation, after reaching a certain critical value, one departs from the first linear curve and goes to another curve, which over the range in which one can plot it is very close to a straight line. Generally the data are such that you can lay a ruler right along it. This has a slope of  $\frac{1}{4}$ , corresponding to a heat flux law which is proportional to the mean gradient in the flow, the thermometric conductivity and to  $\frac{1}{4}$  power of the ratio of Rayleigh number to some critical Rayleigh number. This is the region that has often been called unsteady cellular convection. There are many scales of motion, but it still has a quasi-cellular character, and it proceeds to a Rayleigh number of about  $10^6$ , about 1000 times the critical Rayleigh number. At this point the curve, experimentally, has a very sharp break again. I will discuss some of the theories about these results tomorrow. It breaks to a curve whose slope is a  $\frac{1}{3}$  power. This is a region which we have come to call fully turbulent convection. The motion is quite disordered. You can get  $10^{10}$  Rayleigh numbers in a small bottle of acetone. Hence, I was shocked to hear that the Rayleigh number in the sun is only  $10^{10}$ . In any event, between  $10^6$  and  $10^{10}$ , and beyond to the best of my knowledge, one has what one would call fully turbulent convection. It is interesting to note, that when you have a  $\frac{1}{3}$  power law, the heat flux becomes independent of the spacing of the bounding surfaces. The intermediate region acts as a short-circuit to the flux of heat, the concentrations of the gradient are all confined to the boundary region. Now, may I answer the question? This corresponds in the first instance to just cellular convection and we must then ask about the Rayleigh number of the evolved field. Now strangely enough, in the experiment, we cannot control the effective Rayleigh number because the dimensions of the unstable region are changing. We can control the heat flux, which is another possible experimental parameter, and let the fluid pick its own Rayleigh number. From the dimensions achieved in this first experiment, the depth of the layer was of the order of 10 cm when the total depth was about 20 cm. This yields a Rayleigh number of about  $10^7$ . So the most evolved form of the convection we were looking at was in this region, but by changing the basic parameter, supposedly you can cover both these regions either with quasi-cellular or fully turbulent motion.



— K. H. BÖHM:

It should be added that the Rayleigh number which has been given here,  $10^{10}$ , refers to the thickness of the layer which corresponds only to the most unstable part of the convection zone, assuming a thickness of 500 km for this part. Compute the Rayleigh number for the whole convection zone, you get a number which is much larger. It has usually been assumed that it is correct to compute the Rayleigh number only for the very unstable part of the convection zone, because one believes that the coupling between this layer and deeper-lying layers of the convection zone is small.

— E. SPIEGEL:

In answering the question whether one should look for a mixing length, and continue to apply mixing-length ideas or seek a more elaborate theory, one has very little choice but to try to test the validity of these notions in connection with laboratory experiments on convection, since we cannot hope to do better on the sun observationally. For this reason I would like to mention the connection of the mixing-length ideas with convection theory, and the laboratory results. In the solar convection studies, the mixing length has been taken to be nearly the scale height. But if one looks at the expression for the scale height, one finds that it is roughly proportional to the distance from the surface of the atmosphere. In particular, for the polytropic model it is exactly proportional to the distance from the edge of the star. This is an amusing coincidence with the kind of mixing-length assumption made in the ordinary boundary layer theory, and one might surmise that, if an application of these ideas is made to the laboratory situation, then the natural choice would be to make the mixing length proportional to the distance from the boundary. It is possible then to write a single expression for the closed system relating the temperature gradient to the mixing length. Then one can put in the hypothesis that  $l$  be proportioned to  $z$ . One finds that, away from the immediate neighborhood of the boundary—what TOWNSEND in his experiments has called the boundary sublayer—the dependence of  $T$  goes into a  $z^{-3}$  power law. This is not the same answer as one derives from dimensional analysis. The dimensional analysis has been applied by PRIESTLEY, and he finds a  $z^{-3}$  law, while the experiments by TOWNSEND give a  $z^{-1}$  law. So there seems to be at least in this sublayer a difference in the dependence on  $z$  between the experimental and the mixing-length calculations. One might think that this would suggest trying another kind of mixing-length hypothesis, but I wouldn't know what to suggest at this point. So I think the question is then raised that perhaps near the boundary, in the transition zone discussed by MALKUS, we cannot hope for a precise representation; although one feels very strongly that in the deeper regions the representation by the mixing

length would be fairly adequate. The only question in my mind then would be the difference in opinion between Mrs. BÖHM-VITENSE and MALKUS on the importance of the transition region. I believe myself that the thickness of the transition layer is of importance for the following reason.

When you get into the deeper regions you are essentially in an adiabatic gradient. This is the one you integrate in to the center of the star. Any small error in the gradient could show up as a large error in the temperature derived at the center of the star. However, the adiabatic gradient you get to depends on the thickness of the transition layer. So in that sense I would have thought that the transition layer, at least in thickness, was important. If this is the case, then it is of some importance what the dependence in the sublayer is. It is also clear that the thickness of the layer, in any mixing-length theory, will always be of the order of a few mixing-lengths. Therefore it could never be thinner than a mixing length. I cannot imagine how you could get a structure smaller than a mixing length. So, in that case, in using a mixing length theory, you are essentially putting a lower limit to the thickness of the transition layer by the very nature of the approach used. These are the few ideas I have about trying to test the layer, and I hope Mrs. BÖHM-VITENSE will have a correction for it.

— E. BÖHM-VITENSE:

It seems to me that the main disagreement is in what we call the transition layer. I didn't regard this whole very unstable region as a transition layer. If I talk about a transition layer, I just mean the very upper part of it, only those layers where I get disagreement between the mean value of any physical parameter (taken over one scale height) and the local value at the point which I am just regarding. If you take a point about  $\frac{1}{2}$  scale height below the boundary layer, then the difference between this mean value and the value which you obtain at the point in question is not very large. To check this, for instance, you can calculate the  $\Delta T$ 's by following the upward moving gas starting  $\frac{1}{2}$  scale height below the point considered, up to the point, and then calculate the  $\Delta T$  which you obtain by following the downward moving gas starting  $\frac{1}{2}$  scale height above the point, and then take the mean of these two  $\Delta T$ 's. This you can compare with the  $\Delta T$  obtained from the relations used in our theory. In the region somewhat below the boundary, you will find agreement within 20 or 30 %. But in the very high layers you will find disagreement, and this is the layer in which I think our theory is certainly wrong. This region, I called the transition layer. An error in this region really does not affect very much the adiabatic which the temperature and pressure follows in the very deep regions. An error in the temperature values for the very unstable region, of course, would.

— W. H. McCREA:

Would you tell us what this means in terms of optical depths?

— E. BÖHM-VITENSE:

Optical depth is not a good scale in the convection zone. For the optical depth you would reach values of several hundreds already, when the pressure has only increased by about 50% from the boundary of the convective layer. One should introduce the geometrical depth. I would guess that the region to which I referred as the transition region is about 100 or 150 km thick, but that is just a guess. That is, below  $\tau = 0.8$ , which is the upper boundary of the unstable layer.

— H. LIEPMANN:

I'm afraid I have to make a quite negative statement. I think nobody in aerodynamics believes in mixing-length theory anymore, and hasn't for at least the last ten years, I do not know enough about convection zones, and I like to leave these to somebody more qualified. In aerodynamic shear turbulence the mixing-length theory had in early time one advantage; namely, to put all the factors of ignorance in a length, and it was believed one could imagine a length easier than something else, say like apparent shear. Using this approach, after a while one begins to take the length seriously, and then, of course, one gets into difficulties.

PRANDTL introduced the mixing length by analogy with the mean-free-path of gases. Now a fluid in turbulent motion is anything but a gas. No particle is ever without interaction with its surroundings; turbulent motion is much more analogous to a liquid. If one attempts a viscosity theory of liquids on the basis of a mean-free-path argument, one gets in exactly the same difficulty. So if you like the mixing length, keep it, but do not take it too seriously; *i.e.* if you get lengths small compared with some characteristic length don't worry about it, and if the mixing length goes to zero or infinity it is also no cause for alarm. But any result which you can get from the mixing-length theory, you can get in all cases which I know of, *e.g.* in boundary-layer theory, jets, etc., without the mixing-length concept, from much more general considerations of similarity. I think that eventually one will be able to get rid of this ill-defined auxiliary length and develop the theory more straightforwardly. In boundary-layer theory these days, and I think CLAUSER would be the expert on this point, one uses *e.g.* more general asymptotic considerations, which are essentially similarity considerations. And I think that eventually we will do that here too. I am not prepared to make any suggestions in detail at this time.

Just as a last fly in the ointment: I was a little worried by Spiegel's remark that dimensional analysis gives something else than is observed. This would be against the laws of nature, I think. Dimensional analysis must be right if you've got all the right factors.

— E. SPIEGEL:

I agree that dimensional analysis, done right, can't be wrong. But as it has been done in convection problems, that is, as it has been done by PRIESTLEY, it has given an entirely different power law than Townsend's experiments produced. TOWNSEND worried about this very seriously as you can imagine, and has, as far I know, not been able to discover the cause of the discrepancy. So, I don't know why there is a difference, it's probably dimensional analysis not properly applied: or there may be a factor missing. And I think one amusing factor is that Malkus' theory does give the right tendency towards the boundary.

— W. V. R. MALKUS:

The phrase «dimensional analysis» seems very convincing; you can't have anything wrong. Usually you can't have anything. You find that if you use a complete dimensional analysis you have learned practically nothing. Invariably any use of dimensional analysis and similarity arguments that leads to more than trivial results is also based on some physical assertion about the nature of the flow. So when you say dimensional analysis or similarity arguments can't be wrong, they can't be wrong if your physical assertions are correct. Tomorrow I would like to talk to you about the classical assertions concerning these flows; for instance, assuming that viscous processes are unimportant far from boundaries, one can then show how to apply these same similarity arguments to the convection problem, where they lead to incorrect results. This then requires a reinterpretation, a reassessment of the assertions about the mechanisms which underlie the similarity argument. In doing that we will have to construct new assertions, in keeping with the observations. So I wish to add to Liepmann's comment; dimensional analysis can't be wrong if you say nothing wrong about the physics. But if you make a false assertion, you say that viscosity and conductivity are unimportant somewhere—which might, or might not, be a false assertion—or you assert that the flow depends only on a distance from a boundary, these assertions then lead to results in a quite general way without specifically describing the mechanism. If you don't get experimental results agreeing with these, obviously you are only assessing the validity of your assertions. The general similarity arguments concerning these flows are all constructed in terms of non-dimensional numbers. For example for laboratory-like convection the quantity  $R$  (RAYLEIGH) and  $\sigma$  (PRANDTL)



are the only non-dimensional numbers. That's all you need to know to specify the flow. If you hold  $\nu/\kappa$  fixed and  $R$  fixed, all you learn from the general equations is that the flows will be identical. For sheer flow the corresponding number is the Reynold's number; if you hold it fixed, and keep the same geometric arrangement, you find the flows will be the same. But you don't know what the flows are. Additions to these results, such as the logarithmic velocity laws, are based upon additional physical assertions. It is these assertions, we must assess carefully, particularly when we go to more general situations in a stellar atmosphere where there are more parameters and more physical variables are important.

— F. H. CLAUSER:

I might say a bit on what we know about boundary layers, and interpret that somewhat in the light of Malkus' remarks, which I think would have a certain tie-in with what we know about turbulent boundary layers. If we have flow over a surface and a boundary layer occurs, then there is a layer next to the wall in which viscosity plays a very significant role. If we divide the boundary layer into two regions, an outer region and an inner region, then the experimental results are, that this outer portion, which is fully turbulent, is completely similar as far as profiles and structures of the large eddies are concerned to every other turbulent profile under the same conditions of zero pressure-gradient along the plate. This outer structure, properly taken, is divorced from the wall. Its structure as regards the velocity profile, the big eddies, the energy-bearing eddies, the shear-bearing eddies, and so on are concerned is completely independent of Reynold's number; that is, completely independent of viscosity. If you had some magic way to turn up or turn down the viscosity in this region, you would find no change in the characteristics as far as the large eddies are concerned in this region. Now the boundary layer as a whole does show an effect of Reynold's number, of viscosity, but this is because when you try and fit this outer layer onto the inner layer, a major portion of the velocity jump, and the same is true of the temperature jump, occurs in this laminar sublayer, which is only a minute function of the total layer thicknes.

The Reynold's number dependence occurs primarily because of the insulating layer, insulating as far as heat conduction is concerned, insulating as far as shear transfer is concerned, which occurs.

Now if we were to apply this to Malkus' results, it seems to me that we would have in this turbulent region a transfer taking place, in which every layer that is fully turbulent is similar to every other layer, and we would have relatively slight gradients within them. The transfer in this region is probably very great, but you do have regions in the two boundaries which would differ

depending on the boundary conditions that you meet. I guess that if we put a solid wall on top, and a solid wall on the bottom, again we will have two laminar sublayers, one on top and one on the bottom, and a major portion of the temperature drop will occur in these two laminar layers, one on top and one on the bottom. If I understand Malkus' thought, this is essentially in agreement with observation. Now then, if we free either of these boundaries from a solid wall, as he has done, there is no constraint that zero velocity must occur at a given place; and my guess is that again, if you were to make observations, you would find that there would be a sharp layer, with turbulence inside and non-turbulent flow outside. There is remarkable similarity between the picture that you see when you look at the turbulent wake of a bullet or the turbulent boundary layer of a bullet, and what you see in this picture of granulation in the sun. If you were to free both boundaries, as apparently you do free them on the sun, my guess is that you would apparently have on the lower edge, a sharp but wiggly boundary; and that consequently this layer in between would probably have very sharp edges top and bottom, a turbulent region in between. Above you would have laminar flow, and below you could have laminar flow. If you watched, with time you would find that these protrusions would in fact go in and come out, with a certain massaging motion. It's almost as though you could put a rubber membrane here, and massage it from below, as far as the upper flow is concerned, and the same is true of the lower flow, but you would have this highly turbulent, highly chaotic vortical motion, taking place within the layer.

This last portion is speculation. I have no direct experience with such convection, but I've seen this kind of thing happen with jet jump, and other things so often, that it wouldn't surprise me a bit if this picture would look good. Now, if this is true, I wonder what observational consequences this might have. If, in fact, the upper and the lower edges of this convective layer had a sharp boundary, sharp as far as a turbulent change is concerned, you would not see it if you looked at it straight on. You very well might see it if you looked at it edgewise, with enough resolution. My guess is, from the numbers you've used so far, that you have far from enough resolution, because at present you are just able to see with some clarity the big eddies; and to see this you would have to be able to see the smaller eddies that take place. Otherwise this boundary would just be fuzzed out.

— E. BÖHM-VITENSE:

Where would you expect this boundary to occur? Would you expect it where the motion in the fluid is decelerated or where the boundary of the unstable layer occurs? Note that in our model of the solar atmosphere there is a very smooth transition between convective heat transport and practically

no convective heat transport at all. Where would you expect a transition region between turbulence and non-turbulence?

(*Ed. note:* there followed a confused discussion in which no more information was added than in Clauser's remarks above. The following interchange acts to clarify a bit this attempt to work back and forth between laboratory cases involving solid boundaries and the astronomical case of a free boundary.)

— R. B. LEIGHTON:

I'd like to ask whether it is really clear that one can extend or apply the laboratory situation results to the sun, because there might very well be other parameters that are important. I take it that this very thin boundary sublayer, whatever it is, is one in which viscosity, molecular viscosity, is the thing that determines the flow. Can we really expect viscosity to play a significant role on the sun?

— F. H. CLAUSER:

The laminar sublayer is associated only with a fixed boundary; here, you have both boundaries free—you have no laminar sublayer.

— R. B. LEIGHTON:

Well, the thing that I am worried about, would it be literally viscosity that would define the thickness of the boundary between these two types of flow on the sun? Also, may not the fact that the sun has cell sizes that are comparable to the scale height make a great difference in the type of flow that we have? Will the compressibility, and perhaps other things, play an important role on the sun, whereas they are of negligible importance in the laboratory?

— F. H. CLAUSER:

I haven't made myself clear. In the sun I do not anticipate any laminar sublayer. The laminar sublayer—I brought that in only because I wanted to explain at first what I really know, and that is this case of the boundary layer in which one edge is free and one edge is fixed. Now then, I think that the case that applies in your convective layer with both edges free, would more properly be that of a jet emerging into the atmosphere from an orifice, which has thus all edges free. There, we have no laminar sublayer at all, just a sharp wiggling boundary on both sides.

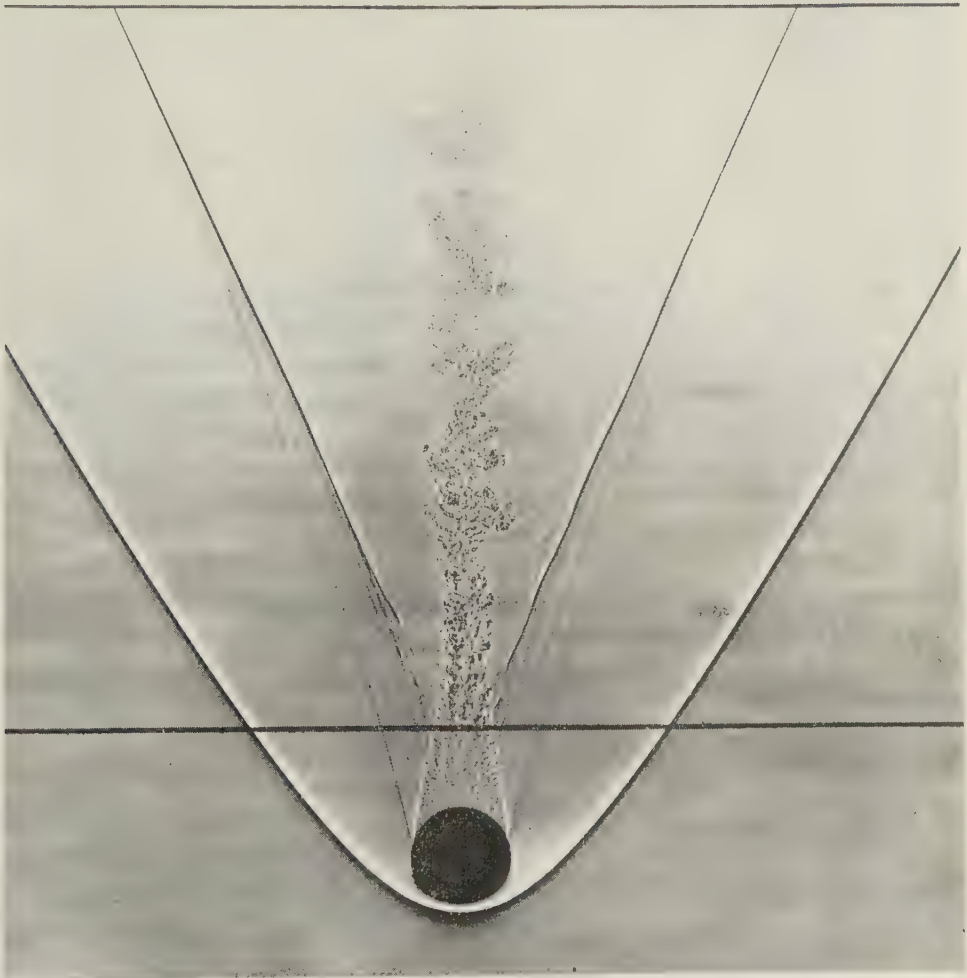


Fig. 1.

(*Ed. note:* see accompanying photograph of a sphere in flight; the turbulent wake corresponds to either the ballistic or jet models mentioned by CLAUSER.)

— S. GOLDSTEIN:

I would raise quite another problem. I am thinking about the granulation on the films we saw yesterday. It appeared that these were certainly motions due to instability. The ordinary Rayleigh theory, for example, for the instability of a thermal layer does not produce a fluctuating phenomenon such as we saw nor, I think, would a fully developed turbulent flow produce the quasi-



periodic fluctuating pattern I saw. I could think of no mechanism whatever by which, if you put in steady boundary conditions, you would get such a picture. It seemed to me that you would be driven to unsteady boundary conditions. I do not know what the boundary conditions are; I do not even know if anybody knows what the boundary conditions are, but whatever they are, if they are steady, we cannot I think ever get the kind of appearance we saw in those pictures. The only way I could think of in which we could get that sort of appearance would be to have unsteady boundary conditions. The question I wanted to ask the astrophysicists was this: Is there a possibility that you can have, at the bottom of what I may call the granulating layer—I do not mean the whole convective layer, but just the granulating layer—a fluctuating temperature with something like the right period? The periods, of course, do not have to be the same; when the calculation is done, harmonics and subharmonics soon will appear. But, in a crude way, if the overturn is about the same as the period of the temperature fluctuation, you will get an instability which will contribute the right kind of fluctuating appearance. That is a lot more, and I am talking now purely of the convective part of the process, not of anything else. The temperature variation does not have to be very large, but perhaps it may be large enough to go through the critical Rayleigh number for the granulating layer. My question is, is such a temperature fluctuation possible? Such a model is interesting in its own right. There are a number of these fluctuating things in nature where you get intermittent instabilities and intermittent turbulence.

— W. V. R. MALKUS:

I want to report, as a geophysicist to the aerodynamicists, some experiments, which have not been very familiar to the aerodynamicist, because his concern has primarily been with shearing flow. This problem of interpreting turbulence only in terms of Clauser's wind-tunnel has a certain danger. Most of us in geophysics and astrophysics come across turbulent flows whose basic energy source is thermal. There are good laboratory experiments, which have been performed, regarding thermal turbulence. I think the aerodynamicists will see in them much of the character they see in their shear turbulences, and the astrophysicists may see in them examples of processes he observes in nature. Now, in direct answer to the question raised by GOLDSTEIN, consider an experiment done between two rigid plates held at fixed temperatures. As CLAUSER anticipated, sharp boundary regions are formed, and we'll explore how they differ from boundary regions one might expect in shearing flows tomorrow. This flow where the Rayleigh number is between  $10^3$  and  $10^6$ , is a quasi-steady, aperiodic motion, with cells that form and persist for only a short time. The characteristic lifetime of a cell is equal to the dimensions

of the cell divided by its velocity. That is exactly the same sort of lifetime as we get for solar convection, too. The characteristic scale of motion even in the fully turbulent situation is comparable to the vertical dimensions of the system. So, for example, a layer like this has motions in it, whose dimensions are roughly the dimensions of the entire system, and it is these motions that have the largest amplitude even though there are a tremendous number of other spectral components. If you look from the top, or from the side, or from anywhere you can into the system, you see motions which are aperiodic, whose characteristic scale is the dimension of the system, whose period is thus  $4d/V$ . I suppose we may be a little incautious in calling them turbulence, so I use the phrase, and I hope it will be acceptable, thermal turbulence. We are in the rest frame of these motions in contrast to shear flows. No one runs along with their instruments keeping up with the mean flow in shearing flow, and so you don't see the evolution of individual elements advected with the fluid. This will make it look different from turbulent shear flow. Still accept it, though, as an example of turbulence, and that the properties of such a flow are so similar to the ones we see in the sun that many of us for many years now have thought there was a very intimate connection. Certainly Mrs. BÖHM-VITENSE has, in even mentioning that there is such a number as this, suggested a similarity and I believe that tomorrow we can provide some convincing evidence that there must be.

— J. TUOMINEN:

CLAUSER said that in the laboratory, the higher resolution we have, the smaller eddies we can see. Has not this come in connection to the slides shown yesterday by SEVERNY? He has so small a resolution that he could not see the granules, but he only saw larger areas of the sun. If we consider a part of the sun, then he found areas with different velocities, upwards and downwards. These areas are much bigger than the granules. Now, if we have a higher resolution, then we see the granules. Perhaps, if we had still higher resolution, we could see still smaller eddies on the sun.

— L. BIERMANN:

I understood GOLDSTEIN to state that if one has strictly stationary boundary conditions, he couldn't see how you would have unstationary conditions in the layer in question. Now suppose that you have the case of a thermal instability, a superadiabatic gradient. Inevitably, no matter how, if you get motions with sufficiently high Reynold's and Rayleigh numbers, you would expect non-stationary features just from the ordinary reasoning of the theory of turbulence. Then necessarily you would get non-stationary features, just of the kind you observe. I'm not aware of any real problem in this area.

— S. GOLDSTEIN:

I'm sorry, perhaps I didn't explain very well. The «period» of the intermittency is the so-called lifetime of the appearance, and is not, so far as I can see, explained on any physical theory that has yet been given.

— W. H. MCCREA:

Don't we have the same intermittency in our own weather?

— S. GOLDSTEIN:

There are two answers. The first is we do not have the same kind of «periodic» intermittency, and the second is that we still have a job to do in meteorology. In detail, for example, there certainly are in meteorology, theories of cloud formation, but what is seen is quite different from this kind of intermittency.

— H. PETSCHER:

If I understand the question correctly, it could be explained by a superposition of different periods. You see, you have periods of the granules, and then superposed on them another period, which you say would have to come from boundary conditions. Now in the sun, as you go down, all of the conditions change, scale height and so on. So that the characteristic frequency for a slightly lower layer is very probably different from the one of the layer that you see. If the motions from there are superposed on the ones which you see, I think one gets exactly the effect that you're looking for.

— C. A. WHITNEY:

Let me summarize how this situation on the upper part of the convective zone looks to me, then comment particularly on the region above the convectively unstable layers, above optical depth unity, in terms of some specific calculations. Some of these thoughts have come from interchange with KROOK and THOMAS.

Below some depth in the solar atmosphere, there is a region that cannot be static. Radiative transfer processes are insufficient to carry all the energy flux from the solar interior, so convective motions set in. Just above the unstable regions the atmosphere is in radiative equilibrium, and if isolated would be static. However, in Clauser's words, it is being massaged from below, so it cannot be static. Because in this interaction region, all apparently agree, the mixing-length representation of the convective zone breaks down, a detailed picture of the interaction region is difficult. However, I think we

can make some comments on the kinematics. Whatever the model, in the penetration region, it will include pressure and temperature fluctuations, which will in turn produce perturbations travelling up into the stable regions. An explicit formulation of this situation by KROOK starts by imagining a plane at some depth, writing all the significant variables—pressure, velocity, etc.—as random, or quasi-random functions of space and time on this plane and then using this plane to define the boundary conditions for the flow in the upper region. In other words the lower region is eliminated, and its effect is simulated by the plane of fluctuations. KROOK has discussed the effects of this type of boundary conditions on the flow above, although there has been very little explicit work done on this model. It is quite obvious that there will be a variety of modes of motion generated.

A point which KROOK particularly emphasizes is that the system must be treated as a whole. We must look for steady-state solutions and must recognize that these regions will be acting on each other. There is a sequence which we might in principle go through. Having solved for the structure of the radiative region under the influence of the convection zone, we then go back and rederive the structure of the convective zone as it is influenced by the modified radiative zone. This process should be repeated to convergence. There are reactions in both directions which may well turn out to be significant.

In the region above this fluctuation plane, the gas is stable against convection, so we might offhand expect the motions to be predominantly of the curl-free or compressive type. There will, however, also be a divergence-free type or gravity wave. Both types will exist, but one's feeling is that perhaps most of the potential energy associated with the wave motion will be bound up in compression rather than gravitational potential. In this situation, when you have waves of both types, it is impossible to weigh what we should expect in the way of phase relations between one quantity and another. It is impossible to say, for example, whether we should expect the rising elements of this region to be hotter or colder than the descending ones.

I might conclude by outlining two ways of looking at the granulation. These are extreme models and clearly the situation lies somewhere in between. A complete treatment along the above lines should provide, among other things, a picture of the granulation. Lacking such a complete treatment, one might look at two extreme models of granulation. One way is simply to forget about the temperature fluctuations in the convective region, and regard the convective motions as equivalent to pistons which produce pressure perturbations. Thus, as above, both acoustic and gravity waves will be produced. We might say that what we see in granulation is the field of acoustic waves generated by the convective zone. A second extreme model is to conceive of the convective motions—below the idealized plane referred to above at the



top of the convective zone—imposing temperature variations, the overlying layer remaining unaffected. In the simplest terms, we would consider the granulation to result from looking down through the overlying atmosphere to the hot and cold gas in the convective zone. We know this is incorrect because the higher temperature associated with the rising element will affect the temperature distribution in the stable region, so that we should modify this simple picture by introducing temperature variations in the stable region.

I would like to summarize some numerical work we have done on the basis of the first oversimplified picture. We took the initial value approach, putting a piston in the solar atmosphere at about optical depth unity, and gave the piston a period of five minutes and a velocity amplitude of one km/s. We wrote the continuity and momentum equations in standard form, including the gravitational acceleration, and restricting ourselves to one-dimensional motion. Since a proper solution of the energy equation including radiation transfer terms is exceedingly laborious, we made the following simplifying assumption. Each atmospheric element was taken to be optically thin and immersed in a radiation bath at a constant temperature. We integrated the equations numerically and obtained the following results.

The temperature, density, and velocity amplitudes of the wave increased rapidly as the wave moved up into the region of decreasing density. The phase relation between the temperature and density within the wave was quite different from that within an adiabatic wave, because the energy loss term is very important under these conditions. In fact, the maximum of the temperature profile within the wave corresponded to the forward portion of the density profile, so that the regions of maximum temperature and maximum rate of compression coincided. The wave gave up its energy to the radiation bath, and by the time the wave had travelled two hundred kilometers its total energy has decreased by about 25%.

The width of the high temperature front of the wave was about 100 km. From this solution of the one-dimensional equations we might construct the following three-dimensional model of granulation. Imagine that the top of the convective zone be replaced by an array of pistons and that each produces a high-temperature region moving up through the atmosphere as described above. If the dimensions and separations of the pistons are about 1000 km, the appearance of an atmosphere disturbed in such a manner will be consistent with the observational features of granulation. Also the concept that we are actually observing the temperature fluctuations within the convection is consistent with observations.

Unfortunately the bulk of the continuum radiation which we observe from the sun is emitted from that limbo region of transition between the stable and unstable layers of the atmosphere, so it is difficult to separate the effects of these regions by observations in the continuum.

— M. MINNAERT:

We heard this morning some quite interesting theories of turbulence. I would like, if possible, to connect these considerations with the astronomical phenomena discussed in the first days of the symposium. The aerodynamicists have warned us that we should not use the term turbulence loosely. So consider for a moment how far the phenomena on the sun may be designated by the term turbulence, real aerodynamical turbulence.

If we review the observational facts they amount to these. In the lower photosphere, we observe temperature differences. Unfortunately, we are not able to measure velocities in this layer, but we see these local temperature differences varying in time—this is granulation. In the higher photosphere, the region where the lines are formed, we observe in the first place local velocity shifts, directly observed, these are the wiggly lines; and in the second place, we have a certain number of spectrophotometric observations from Fraunhofer lines, curves of growth, etc., which also show that there are velocity differences. Only the first are directly observable macroscopic motions, while the second are microscopic.

And now I should like to ask in the first place about the macroscopically directly visible velocities and the probably connected temperature differences of the granules. Can we call this real aerodynamical macroturbulence as astronomers are used to calling it? Is it not necessary, for example, to have vorticity in order to be able to speak about turbulence? What are the conditions which a velocity field should satisfy in order to be called by that name? One may say that it is only a question of terminology; but as soon as you use the term aerodynamical turbulence, that means that the turbulence spectrum will have a certain number of properties which astrophysicists would like to apply. How far is this allowed?

The second thing is, how far are we allowed to speak about microturbulence in the granular layer? I should think that if there is real macroturbulence, then just because of the turbulence spectrum, one may *a priori* expect that there will also be many *minor* turbulent elements, and that also from the aerodynamical point of view microturbulence looks probable.

The same questions have to be put for the higher photosphere, though the answer may be different there. It should be ascertained whether random waves would give the same spectral phenomena as real turbulence.

— R. N. THOMAS:

I would like to put a couple of numbers on the board relating to what WHITNEY has said. As I mentioned earlier in this symposium, we tried some time ago to calculate the heating of the chromosphere by aerodynamic dissipation of the energy of a spicule on the assumption it was a supersonic jet

but gave up because we didn't have any real knowledge of the thermal state of the medium we were trying to work with or the thermodynamic properties of the spicule system. We have spent the last several years trying to get better information on these unknown properties of both medium and spicules, as well as to develop the analytic structure for treating such an aerodynamic system coupling with a radiation field. I would stress the importance of radiative stability in computing the aerodynamic configuration of such an assumed supersonic jet, maybe coming back to this point later in the symposium.

To make decisions on several models discussed by WHITNEY the same knowledge of properties of the medium must be made. So let me make several points. First, I would like to ask what these observed brightness differences in the granulation mean in terms of the distribution with height of the temperature fluctuations. Now this is a numerical calculation that DE JAGER and PECKER suggested a long time ago; so far as I know nobody has done it in detail. Always one says that an observed brightness fluctuation corresponds to a certain temperature fluctuation, not specifying where in the atmosphere this fluctuation occurs. Let us assume a 5% brightness fluctuation. To a first approximation, we can estimate distribution at the disk center and center-limb variation by considering the fluctuations over a spherically-symmetric surface. We find  $\Delta T_e \sim 50^\circ$  at depths everywhere below  $\tau \sim 0.3$  suffices to produce this 5% contrast at the center of the disk. The same is essentially true at  $\mu = 0.6$ . At  $\mu \sim 0.2$ , the contrast would drop to  $\sim 1.5\%$  for the same  $\Delta T_e$  or require  $\Delta T_e$  to extend upward to  $\tau \sim 0.1$  to give the same contrast; at  $\mu \sim 0.1$ , the contrast would be undetectable. If we wish to confine  $\Delta T_e$  to regions below the  $\tau = 0.3$  level, and to detect a granule at  $\mu = 0.1$  (assuming a contrast of 1 to 2% is necessary for detection), then we require  $\Delta T_e \sim 100$  or  $200^\circ$  at  $\tau \sim 0.3$ . To hold the contrast to 5% at the center of the disk, we require, however,  $\Delta T_e$  to decrease rapidly downward. For example, if we set  $\Delta T_e = 200^\circ$  over the interval 0.33 in  $\log \tau$  centered at  $\tau = 0.46$ ,  $\Delta T_e = 50^\circ$  over the same interval centered at  $\tau = 1.00$ , and  $\Delta T_e = 0$  elsewhere, we find contrasts: 5% at  $\mu = 1$ , 8% at  $\mu = 0.6$  and 0.2, and 2% at  $\mu = 0.1$ . Changing  $\Delta T_e$  to  $150^\circ$  in the interval centered at  $\tau = 0.46$ , keeping it at  $50^\circ$  around  $\tau = 1$  and zero elsewhere, we find contrasts: 4% at  $\mu = 1$ , 6% at  $\mu = 0.6$  and 0.2, and 1.5% at  $\mu = 0.1$ .

This is pure numerology. I take an observed intensity distribution and ask, what temperature distribution is compatible with this? I stress this because these observations relate to the regions above the level of convective instability. We are in the region where penetration occurs, in the region where whatever is going to heat the chromosphere is starting from. So this is the aerodynamic boundary condition that one would like to get out.

Let me emphasize that these several alternatives give a different behavior of the granule intensity contrast as we go to the limb. This is a question which

must be solved observationally, but to the best of my knowledge, the data do not yet exist. I certainly hope to stand corrected on this. This question is relevant to many problems. The computation of line profiles; the interpretation of the effects mentioned by MINNAERT, the boundary conditions for the things WHITNEY has talked about, and, lastly, I want to know what this does to the low chromosphere.

A second point is that an empirical analysis of the structure of the atmosphere shows the absence of momentum input to a height of some  $(1000 \div 1500)$  km above the level  $\tau = .01$ . The atmosphere is in hydrostatic equilibrium under the normal solar gravity value, to an accuracy of some few percent. The temperature rises by about  $5000^\circ$ , but there is no momentum input by whatever the mechanism which causes the temperature rise. This is a strong requirement on any kind of aerodynamic theory of the energy input mechanism. We must have an energy source, but it cannot be a momentum source.

— G. ELSTE:

Was the geometrical effect of shielding taken into account in these calculations? The hot and cool regions will screen each other.

— R. N. THOMAS:

All I have really done is use your contribution function method, and a spherically-symmetric distribution of temperature fluctuations. I assume local thermodynamic equilibrium in the continuum, and ask what results from assumed fluctuations in the source-function.

— G. ELSTE:

On the picture WHITNEY roughly sketched, the granulation would consist of bright regions with adjacent darker regions. But the granulation does not *look* this way. The granulation looks like bright patches surrounded by narrow, dark regions.

— R. LÜST:

I would also like to make a remark on this one-dimensional problem, if you want to compare in detail this calculation with observations. It is my experience in connection with two-dimensional computations, including a vertical magnetic field, that the geometrical factor, what you are losing in the sideways direction, is quite severe; therefore the amplitude increase is not as large as one would expect from the one-dimensional computation. I think one should therefore be somewhat careful in directly applying the calculations to observational data.



— E. BÖHM-VITENSE:

I would like to ask how much the density is increased in this wave, because if the increase is not appreciable I don't think you would see this wave.

— E. SCHATZMAN:

I think that when we go from the plane problem to the non-plane problem, we have the following difficulty. For the plane problem, the velocity field is irrotational, but for non-plane waves the motion is not in general rotational-free *e.g.* for waves coming from points distributed on a given layer. I think that it would be interesting to see how at some distance from the source, the waves coming from different portions of the surface will interfere with each other, and will produce a chaotic velocity field which could turn out to be something between a shearing field and compression waves.

— C. A. WHITNEY:

The velocity semi-amplitude increased from 1.5 km/s at the piston to 2.5 km/s at a height of about 200 km above the piston. The density amplitude had reached a factor 1.5 by the time the wave had gone several hundred km. The amplitudes of all perturbations increased with height, although the total energy of the wave decreased.

In answer to the other questions I must agree with those people completely that when we start talking about geometry, these calculations are inadequate. My point in mentioning it was merely to demonstrate some physical effects which had not been mentioned this morning.

— H. LIEPMANN:

The random piston problem has been partially treated by PHILLIPS. It has not been treated yet for the case of a variable density atmosphere with an energy correction in, and Phillip's treatment was a linearized one, but I think the complete treatment can be made. All you have to do is give the space-time correlation of the fluctuations in the plane, and then you can solve the wave equation as an initial value problem with stochastic variables in it. My feeling is that the linearized two-dimensional problem including the density variation should be the next step.

— *General discussion:*

Relative merits of proceeding with any linearized treatment as opposed to a non-linearized treatment. Agreement that linearized problem might give reliable results for the lower parts of the atmosphere.

— E. BÖHM-VITENSE:

If the density variation is only a factor 2, I don't think you would see the feature described by WHITNEY. The optical thickness of this region would be only about 0.1. Also I do not see why you think that my picture of this morning, which viewed granulation as matter rising from below and circulating through the stable layer, would not be able to represent the observations?

— C. A. WHITNEY:

My apologies, I tried to give the impression that both pictures are possible, in terms of present knowledge. Until we get the type of data referred to by THOMAS, there seems to be no way of choosing between them. On your comment about the optical thickness of the high temperature region, I find from my computations that the increment of emergent intensity produced by the wave is 4% at the center of the sun, and 12% at  $\mu = 0.2$ , when the wave lies at  $\tau \sim 0.3$

— L. BIERMANN:

Three points: a general comment on the use of the mixing length in astrophysics; on instability in early-type stars following the work of KIEPENHAHN; and the possibility of observation of the type of oscillation mentioned by LEIGHTON yesterday.

The mixing-length theory as presented by Mrs. BÖHM-VITENSE is mainly used for two purposes: one, to interrelate the several data of observation—size, velocity, lifetimes, and contrast of granulation; the other, to deal with the internal structure of stars and their evolution. Regarding the first, I think it reasonable to say that within the factor two or so associated with the application of this type of theory, there is reasonable agreement between theory and observation. Regarding the second, consider two methods of integration of a stellar model. If we neglect convection and just use the theory of radiative transfer, starting from the theory of stellar atmospheres, we obtain one curve in the  $\log T$ ,  $\log P$  plane. If we make allowance for convection in the way mentioned this morning, we get another curve, giving a lower temperature for the same pressure. These give quite different models for the sun and for the stellar interior. For the sun, it happens that it is not easy to say definitely which model is more correct. It turns out that what we know about stellar evolution from star clusters can only be understood, for their particular part of the H-R diagram, by using the mixing-length theory just in the form it was presented this morning. I think that this one fact shows that there is in astrophysics, entirely apart from anything that was mentioned this morning, something which indicates that the application of the mixing-length

theory is not so far off as one might have guessed. To get to the radiative solution from the convective, you need a mistake in the mixing-length theory—in the dimensionless quantities that enter—by one power of ten or more. Therefore, for most purposes in astrophysics, we would be happy if any error introduced by the mixing-length theory would be less than a factor 2 or so. There are, of course, special questions in which this is not true, but I would simply make the point that our restrictions on the use of the theory are by no means as severe as they are in the discussion of experimental evidence in the laboratory.

The second point concerns KIEPENHAHN's work on circulation in a rotating early-type star. It can be shown quite generally that a star rotating without meridional circulation is in a singular state. An old theorem of von Zeipel shows this for radiative equilibrium; I discussed the case of convective equilibrium at the Stockholm conference a few years ago. The speed of the circulation depends upon the stellar structure; only for very thin convective layers—essentially pure radiative equilibrium—should one expect large circulation velocities near the surface. KIEPENHAHN has attempted to work out numerical results. For the hot supergiants, in which according to the mixing-length theory one should not expect extensive hydrogen convection zones, KIEPENHAHN obtains velocities of the order 1 km/s. We know from observations that such stars have atmospheric turbulent velocities of some km/s, and this proposal is to link them with the meridional circulation.

The connecting argument is that these circulations would be dynamically unstable according to the criterion of REYNOLDS concerning the instability of shearing flow. It can easily be shown that the Reynolds number associated with these motions is exceedingly large, so one should really expect instability of the dynamical variety, not thermal. This is the root of the idea of KIEPENHAHN for accounting for the observed turbulence in this type of stars.

The third remark is short; LEIGHTON mentioned what appeared to be pulsation with a period of about five minutes in addition to a decay, and I just want to point out that this is rather near to the fundamental period of oscillation in the sun's atmosphere. This quantity can be brought into the form  $P = a/g$ , where  $a$  is the velocity of sound and  $g$  is the gravitational acceleration. This period is obviously a minimum in the photosphere, and in this case it is not far from the observed value. It might be worth-while to inquire into the meaning of this.

— K. H. BÖHM:

You said that some of the results of the mixing-length theory are in agreement with observations, and you quoted among other things the size of the granules. I am not quite sure that one can predict the size of the granules

from the mixing-length theory in a convincing manner. We heard this morning that the scale height, which is used as mixing length, is, in a polytropic atmosphere, always proportional to, and of an order of magnitude equal to the distance from the top of the atmosphere. So, depending on your detailed assumptions, you can get elements of almost any size at the surface of the star. In the sun, the ratio of the local scale height to the distance from the surface is about 0.8 in the upper and 0.4 in the lower parts of the convection zone.

— L. BIERMANN:

Let me just refer to a recent detailed discussion of this point in *Zs. f. Ap.* by some of our people.

— J. WADDELL:

I should like to mention some work which PIERCE and I did on the analysis of limb-darkening, as I think it bears on the discussion we have been having. In solar observations we go from the intensity,  $I_\nu(\mu)$ , observed on the disk to the source-function,  $S(\tau_\nu)$ , and then finally we can go to the monochromatic radiation flux,  $F(\tau_\nu)$ . Studies I have made concerning the errors involved in each of these steps indicate that at  $\tau = 10$  one can magnify these errors in the first step by a factor of a 100. When one gets to the monochromatic flux, however, the error of the flux is only a factor of 10 greater than the errors in the observed intensity. The reason for the large error is that the function  $S(\tau)$  is effectively the inverse Laplace transform of  $I(\mu)$ , a risky numerical procedure; on the other hand, the error in the radiative flux  $F(\tau)$  is small because it is an integral over  $S(\tau)$ .

We have computed  $F_\lambda(\tau_\lambda)$  for optical depths as deep as 8 to 10. The value near the Balmer discontinuity is a little uncertain, and we can only go down to a wavelength of 3100 Å, so we know nothing about the ultraviolet. About 30% of the graph is incomplete. However, the uncertainty of the final values of the integrated flux is about 10%. It appears that to within this limit, the radiative flux is conserved down to an optical depth of ten.

— A. UNDERHILL:

This implies that convective transport is not important above  $\tau = 10$ . I would like to ask Mrs. BÖHM whether this is consistent with her work on the convection zone using the mixing-length theory?

— E. BÖHM-VITENSE:

I cannot answer decisively but you can altogether neglect convective flux down to  $\tau = 2$ . The convective flux increases rather smoothly with  $\tau$  and I



don't think these results are inconsistent with theory. I would not think we can draw any conclusions from this, however.

— K. H. BÖHM:

I would just like to say that we are aware that it is a slightly dangerous procedure to derive temperature inhomogeneities from line profiles. The point of view which we would take now is the following one. We are inclined to believe with the British-French group that there are temperature inhomogeneities of the order of  $\pm 260^\circ$ , as has been given at equal optical depth. We would say that they must certainly have an influence on the line profiles, and it is good fortune that if we take into account these temperature inhomogeneities, some of the discrepancy in the theory of the center-to-limb variation of line wings are reduced. But, on the other hand, we are aware that in principle we should have to take a better source function too. So we don't think this is real independent evidence for the *magnitude* of the temperature fluctuation. We just consider it as one argument in addition to the evidence already indicated by direct observations.

There is one other point I would like to mention, which is independent of what I have just said. I think a few numbers which have not been quoted so far could be mentioned in order to state the hydrodynamical problem of the convective zone more clearly. These numbers will show how radically different the situation is from laboratory convection. In the solar convection zone, having a thickness of about 60 000 km, the density varies by a factor  $10^4$ . The conductivity by radiation varies by a factor of at least  $10^3$ ; it varies very rapidly near the upper boundary of the convective zone, and then the variations are much less rapid. Finally a point which I think must have some bearing on the calculation of the currents in the convection zone is that the quantity

$$\frac{dT}{dz} - \left( \frac{dT}{dz} \right)_{\text{adiabatic}}$$

varies by a factor of at least  $10^2$  if we compute the structure of the convective zone using the mixing-length theory which has been quoted. If we use just a radiative model this quantity varies by a factor  $10^4$  or more within the hydrogen convective zone.

— W. H. MCCREA:

As regards this zone, a few years ago I tried the effect of temperature differences of about  $1000^\circ$  to calculate the continuous spectrum of the sun. You can get wonderful agreement with the figures by putting this in! I think ELSTE mentioned this afternoon that when you look obliquely you see through

one temperature to another, and that effects the limb darkening, and it seemed to me at the time you'd get good agreement by taking into account this effect. BÖHM asked about the lines, but I wanted to point out that there are interesting effects in the continuum as well.

— J.-C. PECKER:

I have two points to make. The first is to mention work by Mrs. ROUNTREE-LUSH measuring the mean-square velocity from curve-of-growth analysis. She found for Ti II a value which I think is the extreme value which has been so far found in the sun, a value of 4 km per s. The problem is how to reconcile this value with others. It seems to me that the only way to reconcile them is to make use of the curve-of-growth theory, with *consideration of non-LTE source-function*. My second point is this. I want to report on work by Mlle. CRY, M. LEFEVRE, and myself in Meudon and Istanbul. This work tried to make use of both equivalent widths and central intensities of lines and of their variation from center-to-limb. We tried to correlate these phenomena with the inhomogeneous model proposed by BÖHM. These results can be seen on Fig. 2. At the center of the disk of the sun, one of the Ti II multiplets gives a source-function represented by curve A, one point from each line. If I follow each line from center to limb, I should find again the curve A, if no other effect comes in. Actually, the observational results of LEFEVRE were quite different, the lines behaving as shown by curves B.

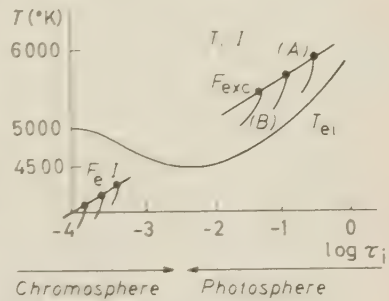


Fig. 2.

Similar measurements have been made at Istanbul on a few multiplets of Fe I, and there the effect was also seen but much smaller. Now, to show how to interpret this, I just want to draw a very quick picture of the results without going into details. If we use a three-column model, as proposed by BÖHM, the optical depth  $\tau=1$  in the line is generally much higher in the cold column than the optical depth  $\tau=1$  in the hot column. So if you look from a certain

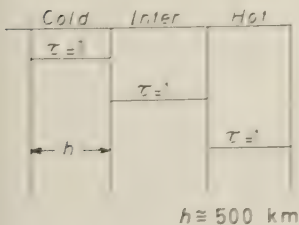


Fig. 3.

direction, not normal to the surface, what you actually see is influenced very much by the size of the elements. If their size was very large at the limb, you would see the same proportion of cold and hot as at the center. But if  $h$  was small enough, you would see only the cold columns. Some computations

have been made using several values for  $h$ , and, as suggested in this discussion by ELSTE, account has been taken of the fact that you penetrate from one column to another. The result is quite striking; it is possible to fit the observed behavior using a value  $h$  of the order of 500 km. Of course, the result depends upon the temperature differences assumed in Böhm's model. If one had been using another model with smaller temperature inhomogeneities, one would have gone to smaller values for  $h$ . This is just an example of what could be done to investigate not only the inhomogeneities, but the size of the elements, from center-to-limb variation, taking into consideration possible departure from LTE.

The question I want to ask now is «What is really the true temperature difference between hot and cold columns?» On this point I just want to mention two things briefly: 1) Measurements by SERVAJEAN at Pic du Midi which agree entirely with the conclusion given by Miss MÜLLER, especially the fact that the correlation between velocity and brightness seems to be very poor. 2) I think that RÖSCH will agree with me that the value that has been given by the so-called French-British school could be too high for a very definite reason: There are actually large scale fluctuations and small scale fluctuations. Some methods of measurement may give large fluctuations of temperature when, around the mean value which is what really counts, you would measure much smaller values. This is the reason why I am inclined to believe the value given by RÖSCH and SCHWARZSCHILD is correct.

— G. ELSTE:

How did you convert the equivalent width of the Ti lines into temperatures? Did you use the linear approximation  $S = a + b\tau$  for the source-function?

— J.-C. PECKER:

No, we used the actual source-function, derived from central intensities.

— G. ELSTE:

Did you assume LTE and then compute the excitation temperature?

— J.-C. PECKER:

Yes, for the first approximation, but then we iterated the solution.

— J. RÖSCH:

I would like to mention several points in connection with things which have been said. First, the question of the value of  $\Delta T/T$  seems an important

piece of data. I wish to mention the difficulties in deriving correct values of  $\Delta T/T$ . Once you have taken a picture, the simplest way is to make a microphotometer tracing through the field. Then you get a curve like this:

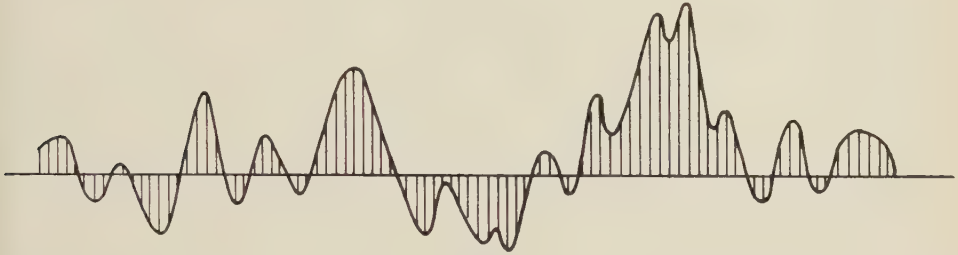


Fig. 4.

Then you take an average curve and compute a r.m.s. deviation from this curve. Doing this you generally find a rather small number. There is another longer way which may give more significant results. You make many such curves or use an isophotometer and make a map of isophotes of the granules looking like this:



Fig. 5.

You may then draw the profiles of individual granules. What you then find is profiles of granules looking like this:

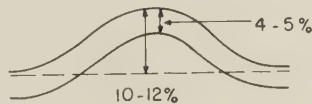


Fig. 6.

We have done this for approximately 60 granules on a picture which was not one of our best. We found differences between maximum and minimum intensity of about 11% of the average—with a displacement of the various granules



by about  $4\%$ . On the same solar field also we made a r.m.s. estimate and found  $3.5\%$ . After this you must correct for the effect of limited resolving power. The scattered light is not very important. We correct these values by  $(11 \div 12)\%$  and find a factor of two must be applied, giving an average  $\Delta I/I$  of—say— $24\%$  total amplitude for a given granule. The maximum fluctuation, not for one granule, but from the darkest to the brightest part of the film is, when corrected, about  $(30 \div 32)\%$ . You see that the result is widely different from  $3.5\%$ ; and if you measure the  $\Delta I/I$  starting only from the r.m.s. you must perform a mathematical analysis to derive from this the total amplitude  $\Delta I/I$ . We avoid this mathematical step by going to the trouble of making isophotal maps, and I think the result is probably better.

From the present values, derived at about  $\lambda 6000 \text{ \AA}$ , one may compute a total amplitude  $\Delta T \simeq 350^\circ$ . This was done with our 23 cm objective and the picture was not one of our best. We are ready to do this with a larger objective now. I expect to find steeper sides on the granules but not a bigger  $\Delta I/I$ .

I would like to comment on the work of SERVAJEAN, who does not find a close correlation between brightness and outward motion. If you consider that there are granules which seem to explode, there appears to be some dark matter just in the middle of a ring of bright matter.

Why shouldn't this dark matter in the middle also be an *ascending* column, so that if you enter it into the analysis it will diminish the correlation? Another point concerns what THOMAS has said about the observations near the limb; he said the observations must decide between the various possible curves showing variation of  $\Delta I/I$  across the disk. I am afraid it will be difficult to decide near the limb because the distribution seems to be rather different. You see a rather smaller number of granules and only the brightest points are visible—separated more widely. It will be hard to define  $\Delta I/I$  and the interpretation will be difficult.

The last point concerns Clauser's comment this morning about the appearance of these motions at the limit of a turbulent layer. He said that if this is turbulence one must see smaller and smaller elements. It seems to me that we can now say that we see bright regions separated by definite dark areas and we do not have a continuous phenomenon. We must try to interpret the size of these things. We may, with better resolving power, find things inside these areas but the fact remains that at least one definite scale exists and we must try to interpret it.

— B. E. J. PAGEL:

This is really in the nature of a short question on the observational side, but I think it is a fairly important one in connection with the various types of motion that have been discussed. SEVERNY mentioned yesterday that there

are several scales of motion, and I am not quite clear which motions are on which scales. First, there seems to be the  $10^4$  km scale, rather persistent fields of motion—several hours—which might perhaps be connected with meridional movements. I think this has been suggested in the past and BIERMANN brought it up again this afternoon. Then there are the «wiggles» which, if I understand correctly, are on a scale somewhat larger than the classical granulation—about 3 000 km, I think. I don't know if there is any evidence on the lifetimes of these wiggles. I should presume—I'd like to be corrected if I am wrong—that the oscillations observed apparently have a lifetime of 20 minutes. Finally we have granulation, which is less than 1 400 km in scale, with lifetimes of the order of 8 minutes. Down to here we seem to have distinct phenomena which are not affected by the resolution of the equipment. Perhaps below here, limited resolving power comes in. I would be glad to know if this picture is consistent with the observational material.

— R. B. LEIGHTON:

With respect to the large scale structure, which I would call greater than  $10^4$  km; the lifetimes of several hours refer only to horizontal motions, as far as our own observations are concerned. These are things that we think must be the divergent streaming along the surface of matter which must have come up from underneath. We do not see it coming up for some reason that is, I think, connected with the observational technique—I'm not quite sure. At any rate, the several-hour lifetime is associated with *horizontal* motion. I understand that SEVERNY has found large scale motions, with a vertical component of somewhat smaller velocity amplitude than we find, which also have lifetimes of several hours. We have no information about that. I think it is probably true that the «wiggles» and our oscillations in the vertical motions have essentially the same scale. However, as far as our observations are concerned, I would designate the scale not as 3 000 km, but as *greater than* 3 000 km, this being the *lower limit* imposed by our resolving power. I think it is significant that over a very wide range of wave numbers there is a single frequency that the sun picks out. Concerning the granules we have as yet no information.

## PART IV.

### Considerations on Localized Velocity Fields in Stellar Atmospheres: Prototype — The Solar Atmosphere.

#### B. - Consideration of Convective Instability from the Viewpoint of Physics.

##### Summary-Introduction:

Similarity arguments for fully developed turbulence.

W. V. R. MALKUS

*Woods Hole Oceanographic Institution - Woods Hole, Mass.*

##### Introduction.

In the study of turbulent flows similarity arguments are used to explore the consequences of non-mechanistic assertions concerning the general behavior of the flow. For example, it is currently assumed that viscosity plays no role in the determination of the mean velocity profile of turbulent shearing flow far from a boundary. The consequences of this assumption are that the amplitude of the mean velocity will be determined by the momentum transported into such a region and that the velocity profile will be a solution to Euler's equations.

The first section of this work will attempt to critically re-assess the experimental results used to support the assertion underlying conventional similarity theories. The second section discusses alternative assertions from which the qualitative experimental results can be deduced. A final section outlines the quantitative theory which has been constructed within the framework described in section two.

1. - Two quite different turbulent flows will be explored in order to test

the generality of the conventional and new assertions. The first of these is turbulent shearing flow between parallel surfaces. The second is the turbulent convection of heat between horizontal surfaces.

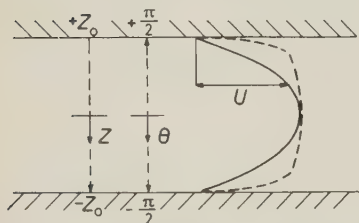


Fig. 1. - The geometry for turbulent shearing flow.

*Current similarity arguments for shear flow.* - In Fig. 1 the  $x$  axis is the direction of the mean velocity  $U = U(Z)$ . The solid velocity profile represents the parabolic solu-

tion for laminar flow. The dashed line represents a mean velocity profile for the turbulent regime. For incompressible steady state flow the momentum transfer per unit mass is

$$(1) \quad \tau = \nu\beta + \overline{wu},$$

where

$$\tau = \frac{Z}{Z_0} \tau_0, \quad \tau_0 = - \left( \frac{Z_0}{\varrho} \frac{\partial \bar{P}_0}{\partial x} \right), \quad \beta \equiv - \frac{\partial U}{\partial Z},$$

$\nu$  is the kinematic viscosity,  $\varrho$  is the density,  $Z_0$  the channel half-width,  $\partial \bar{P}_0 / \partial x$  is the downstream pressure gradient at the boundary,  $w$  is the cross stream velocity fluctuation,  $u$  is the downstream velocity fluctuation and the horizontal superscript bar indicates an ensemble average. This flow is determined by the Reynolds number  $R \equiv Z_0 U_m / \nu$  if  $U_m$  is fixed, where the subscript  $m$  indicates an average over the entire flow. The flow is determined by the alternate Reynolds number  $R_\tau \equiv Z_0 U_\tau / \nu$  if the downstream pressure gradient is fixed, where  $U_\tau \equiv \sqrt{\tau_0}$ . The mean velocity profile is written then either as

$$(2) \quad U = U \left( R, \frac{Z}{Z_0} \right) \quad \text{or} \quad U = U \left( R_\tau, \frac{Z}{Z_0} \right).$$

Among the first observations on this turbulent flow was the discovery of the « velocity-defect law » shown in Fig. 2. At high Reynolds numbers all the data can be fitted to this one non-dimensional curve. The curve is logarithmic beyond a small linear « boundary layer », becoming parabolic in the mid-regions of the flow.

A recent presentation of the similarity arguments is given by TOWNSEND (1958). This argument is based on two general assertions. The first assertion is that viscosity plays no role in the determination of the mean profile far from the boundaries. From eq. (2) for  $R_\tau$  fixed, the mean velocity in the midregions of the flow is then

$$(3) \quad U_c = U_{\max} + U_\tau F \left( \frac{Z}{Z_0} \right),$$

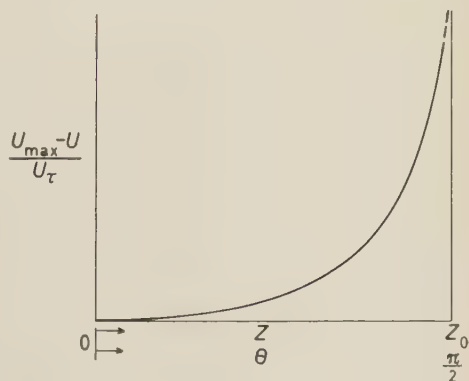


Fig. 2. - The velocity-defect « law ».



where an arbitrary velocity of translation is chosen as the maximum velocity and  $F$  is an arbitrary function of  $Z/Z_0$ . The second assertion is that flow near the wall is only a function of distance from the wall and independent of the (large) channel half-width,  $Z_0$ . Defining a «running» Reynolds' number

$$(4) \quad R_{z'} = \frac{Z' U_\tau}{\nu}, \quad Z' \equiv Z_0 - Z,$$

eq. (2) is re-written without loss of generality as

$$(5) \quad U = U_\tau G \left( R_{z'}, \frac{Z'}{Z_0} \right),$$

where  $G$  is an arbitrary function of its arguments. The consequences of the second assertion is that the dependence on  $Z_0$  in eq. (5) must vanish. Hence near the boundary,

$$(6) \quad U_B = U_\tau G(R_{z'}).$$

It is argued, that in the region of overlap, the boundary law, eq. (6), and the mid-region law, eq. (3), must be the same. Only one choice for  $F$  and  $G$  is then possible, and the overlap law becomes

$$(7) \quad U_0 = A + B \ln(R_{z'}),$$

where  $A = A(R_\tau)$  only and  $B$  is a universal (Von Karman's) constant.

The experiments indicate that eq. (7) holds not just for some small overlap region of the flow but for most of the profile. It has been believed that this fact establishes the correctness of the similarity assertions. However, one might also interpret the experimental results as indicating that the first assertion is incorrect and that a region of completely inviscid flow does not exist. This possibility will be explored shortly.

*Current similarity arguments for turbulent convection.* — In Fig. 3 the solid

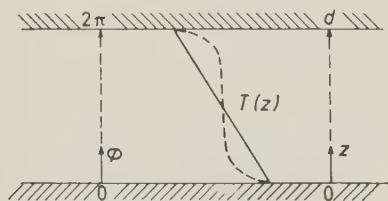


Fig. 3. — The geometry for turbulent convection.

line represents the temperature profile which would exist in the absence of motion between horizontal conducting plates separated by a distance  $d$ . The dashed line represents a mean temperature profile for the turbulent regime. For flows with a small total temperature drop,  $\Delta T$ , the kinematical heat flux is

$$(8) \quad H \equiv \frac{\mathcal{H}}{\varrho_m C_0} = \kappa \beta + \overline{WT}, \quad \beta \equiv -\frac{\partial \bar{T}}{\partial Z}.$$

where  $\mathcal{H}$  is the actual heat flux,  $\rho$  is the density,  $\kappa$  is the coefficient of kinematic thermal conductivity,  $C_v$  is the specific heat of the fluid at constant volume,  $W$  is the vertical velocity and  $T$  is the temperature. For a fixed  $\Delta T$ , this flow is determined by the Prandtl number  $\sigma \equiv \nu/\kappa$  and the Rayleigh number  $R = \alpha g \Delta T d^3/\kappa \nu$ , where  $\alpha$  is the coefficient of expansion of the fluid and  $g$  is the acceleration of gravity. Alternatively, for fixed heat flux, the Prandtl number and  $R_H = RH d/\kappa \Delta T$  determine the flow. Hence the mean temperature profile may be written either as

$$(9) \quad \bar{T} = \bar{T} \left( R, \sigma, \frac{Z}{d} \right) \quad \text{or} \quad \bar{T} = \bar{T} \left( R_H, \sigma, \frac{Z}{d} \right).$$

A similarity argument for turbulent convection was given by PRIESTLY (1954). Paralleling the shear flow study, the assertion was made that viscosity and conductivity play no role in the determination of the mean temperature profile far from the boundaries. The consequences of this assertion were sought by establishing the possible dimensional relations in an equation such as eq. (9). Beyond the boundary region

$$(10) \quad (\bar{T}(Z) - T_m)^{\frac{1}{d}} = \text{const } (Z)^a (\mathcal{H})^b (\alpha g)^c (C_v)^d,$$

where relations between the powers  $a$ ,  $b$ ,  $c$  and  $d$  are to be found so that the right side of eq. (10) has the dimensions of a temperature. In contrast to the shear flow study, PRIESTLY discovered that a unique result is obtained for each of these powers. This is

$$(11) \quad (\bar{T}(Z) - T_m) = \text{const } (\mathcal{H}^2/\alpha g C_v^{\frac{1}{2}})(Z)^{-\frac{1}{2}}.$$

In turbulent convection, then, an explicit form for the « inviscid » region is found without studying the overlap with a « boundary » region.

The experimental evidence in the laboratory (TOWNSEND, 1959) does not support the  $Z^{-\frac{1}{2}}$  law of eq. (11) but fits a  $Z^{-1}$  law rather closely. Hence the correctness of the assertion that molecular transport coefficients are unimportant in the body of the flow is in doubt.

2. - The similarity argument to be advanced in this paper rests on two general assertions concerning steady state turbulent flows. The first of these assertions is that the mean profiles (of temperature and velocity) approach but never exceed, the local condition for marginal inviscid instability.

The problem of turbulent convection will be treated first. The condition for inviscid instability in convection is that

$$(12) \quad \beta > 0,$$

that is that there be lighter fluid below and heavier fluid above. Thus the first assertion may be written

$$(13) \quad \frac{\kappa\beta}{H} = I^2 \geq 0,$$

where  $I$  is to be represented by its Fourier expansion

$$(14) \quad I = \sum_{n=-\infty}^{+\infty} I_n \exp[in\varphi], \quad 0 \leq \varphi \leq 2\pi,$$

where the co-ordinate  $\varphi$  is shown in Fig. 3 and where  $I_n = I_{-n}^*$  in order that  $I$  be real.

The second assertion concerning steady state turbulence is that the smallest

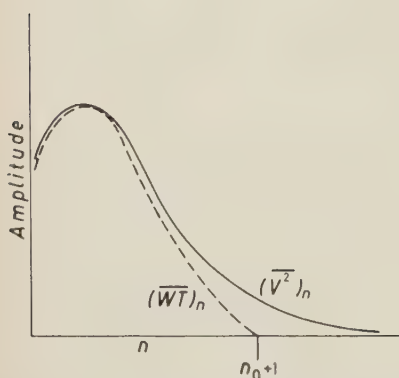


Fig. 4. - Characteristic spectra for « organized »  $(\overline{WT})_n$  and « disorganized »  $(\overline{V^2})_n$ , moments in turbulent convection.

scale of motion effective in the transport of heat (or momentum) is a monotonically increasing function of the Rayleigh (or Reynolds) number of the flow. In Fig. 4 a possible spectral description is given of the mean squared velocity and convective heat transport at some fixed Rayleigh number. The wave number  $n_0 = n_0(R, \sigma)$  is defined as the largest wave number which contributes to the transport of heat. That is, the wave number  $(n_0 + 1)$  makes no effective contribution to heat transport. It is possible, indeed necessary to the Kolmogoroff-like studies of isotropic turbulence, that the « disorganized » spectrum of the mean squared velocity extend to higher wave numbers

than the « organized » correlations responsible for convection.

From eq. (8)

$$(15) \quad \frac{\beta}{\beta_m} = 1 + \frac{(WT)_m - W\bar{T}}{\kappa\beta_m},$$

hence a consequence of the second assertion is that the spectrum of  $I$ , eq. (14), terminate at some  $n_0 = n_0(R, \sigma)$ . We now wish to establish the conditions under which the restricted sum

$$(16) \quad I(n_0, \varphi) = \sum_{n=-n_0}^{+n_0} I_n \exp[in\varphi]$$

leads to qualitative laws for the mean temperature profile as  $R$  (and  $n_0$ ) approach infinity.

One such condition on the structure of  $I_n$  can be found by summing eq. (16) by parts:

$$(17) \quad \sum_{n=-n_0}^{+n_0} I_n \exp[in\varphi] = \left( I_{n_0} \sum_{n=-n_0}^{+n_0} \exp[in\varphi] \right)_r - \frac{1}{\exp[i\varphi] - 1} \sum_{n=-n_0}^{+n_0} \Delta I_n \exp[in\varphi].$$

If then  $I_n$  is smooth in the sense that

$$(18) \quad \Delta I_n \simeq \frac{I_n}{n_0},$$

the first term on the right of eq. (17) is an asymptotic « law » of order  $n_0$  larger than the second term, for  $\varphi \gg \pi/n_0$ .

Before investigating this consequence, an alternate statement of the conditions will be explored. If one writes

$$(19) \quad I_n = G\left(\frac{n}{n_0 + 1}, n_0\right),$$

and expands  $G$  in a power series

$$(20) \quad G = \sum_{m=0}^{\infty} G_m \left( \frac{n}{n_0 + 1} \right)^m, \quad G_m = (-1)^m G_m^*,$$

it is possible to perform the partial sums and explicitly order terms in  $l/n_0$ . The sum

$$(21) \quad \sum_{n=-n_0}^{+n} \exp[in\varphi] = \frac{\exp[i\varphi(n_0 + 1)]}{\exp[i\varphi] - 1} + \frac{\exp[-i\varphi(n_0 + 1)]}{\exp[-i\varphi] - 1} = \Phi,$$

permits one to determine the sums

$$(22) \quad \sum_{n=-n_0}^{+n_0} n^m \exp[in\varphi] = (-1)^m \frac{\partial^m \Phi}{\partial \varphi^m} = \\ = (n_0 + 1)^m \left\{ \frac{\exp[i\varphi(n_0 + 1)]}{\exp[i\varphi] - 1} + (-1)^m \frac{\exp[-i\varphi(n_0 + 1)]}{\exp[-i\varphi] - 1} \right\} - \\ - m(n_0 + 1)^{m-1} \left\{ \frac{\exp[i\varphi(n_0 + 2)]}{(\exp[i\varphi] - 1)^2} + (-1)^m \frac{\exp[-i\varphi(n_0 + 2)]}{(\exp[-i\varphi] - 1)^2} \right\} + \dots$$



Hence

$$(23) \quad I(n_0, \varphi) = \sum_{m=0}^{\infty} \left[ G_m \left\{ \frac{\exp [i\varphi(n_0+1)]}{\exp [i\varphi] - 1} + (-1)^m \frac{\exp [-i\varphi(n_0+1)]}{\exp [-i\varphi] - 1} \right\} \right. \\ \left. - \frac{m G_m}{n_0+1} \left\{ \frac{\exp [i\varphi(n_0+2)]}{(\exp [i\varphi] - 1)^2} + (-1)^m \frac{\exp [-i\varphi(n_0+2)]}{(\exp [-i\varphi] - 1)^2} \right\} + \dots \right. \\ \left. + \dots + \frac{m^a G_m}{(n_0+1)^a} \{ \} + \dots \right].$$

One obvious, but strong, condition that, away from the boundaries, the leading term be of order  $n_0$  larger than all other terms in eq. (23) is that  $G$  be a finite polynomial. The conditions that  $I_n$  be smooth, eq. (18), or that it be properly represented by a finite polynomial therefore lead to the same law at large  $R$ . One may write

$$\sum_{m=0}^{\infty} G_m \equiv g_r + i g_i$$

then from Eq. (20) and (23)

$$(24) \quad I_{n_0 \rightarrow \infty} = g_r \frac{\sin (2n_0+1)(\varphi/2)}{\sin (\varphi/2)} - g_i \frac{\cos (2n_0+1)(\varphi/2)}{\sin (\varphi/2)}.$$

Our physical problem requires that  $\beta$  be symmetric around the mid-point of the region. Therefore either  $g_i$  or  $g_r$  must be zero. If one chooses  $g_i = 0$  and defines  $\theta = \varphi - \pi$  then

$$(25) \quad \left( \frac{\kappa \beta}{H} \right)_{n_0 \rightarrow \infty} = I^2 = g_r^2 \frac{\cos^2 (2n_0+1)\theta}{\cos^2 \theta}.$$

Integrating eq. (25) to obtain the temperature field one finds

$$(26) \quad \frac{\kappa(T_m - \bar{T}(\theta))}{H} = \frac{g_r^2}{2} \left( \lg \theta + 0 \left( \frac{1}{n_0} \right) \right),$$

outside a region within  $\pi/n_0$  of the boundaries. An identical law results for  $g_r = 0$  and  $g_i \neq 0$ .

Near the boundary eq. (26) leads to

$$(27) \quad T_m - \bar{T}(Z) \simeq Z^{-1}$$

in keeping with the experimental results.

The preceding arguments for thermal turbulence are easily adapted to the shear flow problem. The sufficient condition for inviscid stability in parallel

flow is that the curvature of the flow do not change sign. Paralleling eq. (13) one may write

$$(28) \quad \frac{\hat{c}^2 (U/U_\tau)}{\hat{c} (Z/Z_0)^2} = I^2 \approx 0.$$

Since the conditions on  $I$  are just those of the thermal problem,

$$(29) \quad I_{n_0 \rightarrow \infty}^2 = g_\tau^2 \frac{\cos^2 (2n_0 + 1)\theta}{\cos^2 \theta},$$

where  $n_0$  is now a monotonically increasing function of the Reynolds' number. Integrating eq. (29) twice one obtains a velocity-defect law

$$(30) \quad \frac{U_{\max} - U}{U_\tau} = \frac{1}{2} g_\tau^2 \left\{ \ln \left( \frac{1}{\cos \theta} \right) + 0 \left( \frac{1}{n_0} \right) \right\}.$$

This profiles adheres closely to the experimental results (LAUFER, 1950) not only in its logarithmic behavior near the boundary but in its parabolic character in the mid-regions of the flow.

The possibility remains that the heat and momentum transport spectra could have «tails» extending well beyond  $n_0 + 1$ . It has been found that a weak exponential «tail» beyond  $n_0$ , responsible for only one percent of the total transport, can significantly modify eq. (24) for  $I$ . Hence one must conclude that the available experimental evidence supports the second assertion as well as the first.

**3.** — The qualitative conclusions, eq. (26) and (30), have been obtained in two previous studies (MALKUS, 1954, 1956). However they were immersed in the complexity of a quantitative analysis and it was not clear at that time whether these «laws» were immediate consequences of the basic assertions or whether they resulted from the several mathematical approximations. The formulation of the problem in Section 2 was made to isolate these asymptotic consequences of the two assertions from two more explicit assertions on which the quantitative theory rests.

The first of these more explicit assertions is that the smallest scale of motion contributing to the transport of heat (or momentum) is that smallest motion which is unstable on the mean profile. This statement replaces the second assertion of Section 1. It is based on the belief that there is a negligible transfer of organization down the spectrum by non-linear processes. Hence  $n_0 + 1$  is to be found by a conventional stability analysis of the mean profile.

Still, within the constraints so imposed on the fields of motion, many pos-

sibilities remain. The second of the more explicit assertions was that the constrained flow would approach that extreme state which maximized the total dissipation rate. The determination of this extreme state proves to be a tractable, if difficult, variational problem for the optimum  $I_n$ . With  $I_n$  quantitatively determined, a mean field of flow from boundary to boundary and the dependence of the transport on the boundary conditions is given. Comparison with experiment can then establish the range of validity of the assertions with little opportunity for self-deception.

First attempts at the determination of the extreme states for shear turbulence and convective turbulence have been made (MALKUS, 1954, 1956). The quantitative results for Von Karman's constant ( $\frac{1}{2}g_r^2$  in eq. (30)), agree with the data of LAUFER (1950). The quantitative results for the convective constant ( $\frac{1}{2}g_r^2$  in eq. (26)) is twenty percent less than the value found by TOWNSEND (1959). However, in this latter case, more mathematical care must be taken to satisfy boundary conditions and more data must be gathered. A report on steps in both these directions and a new simplification of the mathematical problem will be presented in a forthcoming study.

\* \* \*

This paper was performed under the auspices of the Office of Naval Research and is Contribution No. 1148 from the Woods Hole Oceanographic Institution.

#### REFERENCES

- LAUFER, J., 1950, *Nat. Adv. Comm. Aero.*, Wash., MO. 2123.  
 MALKUS, W. V. R., 1954, *Proc. Roy. Soc.*, A 225, 196.  
 MALKUS, W. V. R., 1956, *J. Fluid Mech.*, 2, 521.  
 PRIESTLEY, C. H. B., 1954, *Austral. J. Phys.*, 7, 176.  
 TOWNSEND, A. A., 1958, *The Structure of Turbulent Shear Flow* (Cambridge).  
 TOWNSEND, A. A., 1959, *J. Fluid Mech.*, 5, 209.

## PART IV.

### Considerations on Localized Velocity Fields in Stellar Atmospheres: Prototype — The Solar Atmosphere.

#### B. - Consideration of Convective Instability from the Viewpoint of Physics.

#### Discussion.

*Chairman:* G. K. BATCHELOR

— R. N. THOMAS:

Do I understand correctly that your  $n_0$  should give a lower limit to the observed size of granulation, if your considerations are applicable to that problem? If I am correct, would you say how to compute this quantity?

— W. V. R. MALKUS:

You are referring to two different parts of this study. The first development refers to the question of the asymptotic consequences of the spatial structure as  $n_0$  approaches infinity. So as far as spatial structure is concerned, we do not define or compute  $n_0$ . The second part—done extremely briefly, and only verbally—was to identify  $n_0$  with the smallest scale of motion that is marginally unstable on the mean field. That's a stability problem. You have to know the mean field or deduce the optimum mean field to determine  $n_0$ . Such stability problems have been solved. In principle they can be solved for the solar atmosphere. BÖHM has solved one recently in a particular form for the polytropic atmosphere. I think others can be done. They will establish a smallest scale, and they will then also tell us something about the energy transfer we can expect due to motion. Note that you will not see the smallest scale. It will be a lower limit. The final spectrum has almost all of its energy in the big motions; practically none in the small motions.

— R. N. THOMAS:

Is it correct to say that  $n_0$  is still the smallest thing that I should see and if I see something smaller than that, I should be unhappy?



— W. V. R. MALKUS:

I think so—unless what you see is isotropic. This  $n_0$  is the smallest motion responsible for a transfer of heat.

— E. SPIEGEL:

I would like to reply to Thomas' question. This theory says that  $n_0$  is the smallest scale that transfers heat. We can, in principle, still see fluctuations (either granules or velocity) at scales much smaller. The  $n_0$  is only the smallest scale for which there is a velocity-temperature correlation; there is no reason so far why there should not be velocity and temperature fluctuations at smaller scales.

— W. V. R. MALKUS:

In the laboratory these smaller scales are not observed. I do not know the full story here. I should guess it tells us something about the strength of non-linear transport down the spectrum. But if we take this  $n_0$  and compare it with the laboratory flows—LAUFER has looked into the shear flows, TOWNSEND has looked into the convection flow—there are no observable motions above the background noise smaller than this  $n_0$ ; but there may be smaller scales in more complicated turbulent processes.

In the central regions of the shear flow, LAUFER draws a picture looking like this for the energy spectrum and then he draws  $UV$  correlations near but not at the center of the flow and it looks like this:

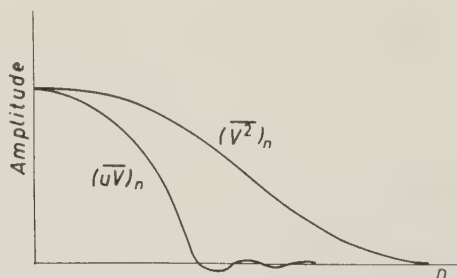


Fig. 1.

However, this does not establish that the energy spectrum for the whole flow goes to higher wave numbers than the transport spectrum in such a flow as this; the stress of necessity vanishes in the mid-regions. Due to external constraints there can be no torque on the fluid as a whole. What we must compare is the smallest scale of motion in the mean flow, which exists near

the boundary of the region—in the so-called «laminar» sub-layer—and determines  $n_0$ . Then we ask if that is smaller or larger than any other spectral component in the flow. LAUFER has some lovely graphs of those data—the boundary is very sharp as you know. Its dimensions are roughly the smallest scale of motion responsible for momentum transport. LAUFER studies this region right to the middle of the boundary layer. He measures the horizontal Fourier components of the motion. He discovers no motion which is smaller in dimensions than this spacing  $Z_0/n_0$ .

There is no motion in the flow whose scale is smaller than the dimensions of the boundary of the region and that is the essence of this theory. The smallest motion in this flow transports momentum.

— G. K. BATCHELOR:

One question which bothers me, and possibly the astrophysicists, is that the rigid boundary plays a dominant role in Malkus' whole theory. The effects of conduction and viscosity that he deduces by his arguments are due to the presence of the rigid boundary. One cannot help wondering what would happen if there was no rigid boundary present. In an astrophysical situation, such as a stellar atmosphere, one might idealize the problem by introducing artificially a rigid boundary as a lower boundary; or, one might, I suppose, choose to think about an atmosphere that extends over many scale heights and try to do without any rigid lower boundary. I do not know if anybody has actually worked out the details of such an approach—a conversation which I had with SPIEGEL suggests that it is being looked at. In that case I do not quite see how the smallest scale of motion—that represented by  $n_0$  in Malkus' argument—could come in and I do not see the connection between the physical processes in those two cases of convection with and without a lower rigid boundary.

— L. BIERMANN:

A point of interest underlying Malkus' discussion is how general are the assumptions underlying the formulae used in astrophysics, as discussed in the last section. I looked into these questions in 1935, and reached two general conclusions. First, the scale most effective in transporting heat is the largest one compatible with exterior conditions. Second, the velocity behaves in such a way that for fixed temperature difference, the heat flow is a maximum. This is of some interest, because in irreversible thermodynamics, the contrary is done. But it seems that in problems of this sort, the turbulence and possibly other mechanisms adjust themselves in such a way that, when you fix the boundary conditions, the production of entropy is maximized. So I would

ask whether, if one applied such considerations as that presented by MALKUS, you would not end up with essentially the astrophysical formulae—to within a correction of a factor 2 or  $\pi$ , etc.—or whether you get something widely different.

— E. SPIEGEL:

There are two problems which have come up on which I would like to comment. The first is that of turbulent convection in a layer whose thickness is much less than the scale height of density. This is the problem discussed by MALKUS. The second problem is that of applying theories of turbulent convection to astrophysical situations. In the discussion yesterday I tried to allude to one of the difficulties of this second problem, that we do not have walls bounding astrophysical convection. However, we do have what might be called soft walls; that is, we have stable layers bounding the unstable ones, and this seems to give some boundary-like behavior to the motions. I cannot discuss these problems in detail in a short time, so I will summarize the Princeton work in general qualitative terms.

Let me begin by discussing our work on turbulent convection in a layer whose thickness is much less than one scale height. In the absence of motion the temperature profile is linear, but the advent of convective motion distorts the profile—cf. Fig. 2 in Malkus' talk. Convective heat transfer causes distortion of the profile so that the temperature gradient, hence heat conduction, is small in the body of the fluid and large near the boundaries.

Let us now imagine the velocity and temperatures fields to be expanded in some suitable set of orthogonal modes satisfying the boundary conditions. If we study the dynamics of one of these modes of the system, we find it convenient to speak of three processes acting. The first is the buoyancy force which results from the mean (ensemble average) temperature profile. The term in the equations describing this force may be regarded as linear once you specify the temperature profile. The second process is the non-linear interaction of the mode with all the others separately. And finally, there is the effect of the viscous forces.

We are interested in statistically steady convection, and therefore look for a statistical balance of these three forces. The main difference with Malkus' theory lies here. He does not treat the non-linear interaction explicitly, but instead has an ingenious way of seeking the net result of the non-linearity by applying integral constants. But, in the work by myself, LEDOUX and SCHWARZSCHILD the non-linear interactions are explicitly considered and are made to balance against the quasi-linear buoyancy term and the viscous term. The procedure outlined is naturally carried out by iteration. We make a guess

at the mean temperature profile, and compute all linear inputs for all modes. Then we approximate the non-linear interactions and solve the balance equation for the relative amplitudes of the modes. It is then possible to compute a correction to the profile and to repeat the process, though to date we have completed only the first iteration.

The immediate question then is, how do you represent the non-linear interaction? Our feeling has been that if there is a tendency for the fluid to reach a preferred steady state—perhaps in the sense MALKUS has mentioned—the particular form of the non-linear term should not be important so long as the essential physics is contained. We have tried to use the best possible form for the non-linear term available from turbulence theory which may be reasonably tractable. In my opinion, the best representation is contained in the recent work of KRAICHNAN; but at the moment this is a more difficult representation than we are prepared to cope with. We have, therefore, in the current formulation of the work used the ideas suggested in Heisenberg's heuristic theory of turbulence.

One new point that comes up is that the interaction terms have not been approximated before in the case of anisotropic turbulence. To handle this difficulty we have made the specific assumption that the anisotropy of the motion is that of the most unstable mode for any scale. The most unstable mode is the one which derives energy most effectively from the buoyancy forces.

This in a rough way summarizes the physics which we have put into the problem. We have actually made the application only for small values of  $\sigma R/8\pi^4$  where  $\sigma = \nu/\alpha$ ,  $R = g\alpha\beta d^3/\chi\nu$  and the definitions of symbols are those used by MALKUS. Let me remind you that  $R$ , the Rayleigh number, measures the ratio of buoyancy force to viscous force and that  $\sigma$ , the Prandtl number, is about  $10^{-5}$  in the photosphere since  $\alpha$  is determined by radiative processes. This approximation simplifies one of the main difficulties in the convective processes—a difficulty which we are now trying to deal with and which I would like to discuss briefly at this point.

In studying the convective motions we have expanded the velocity and temperature fields in terms of a complete set of orthogonal functions. In the approximations we have considered, these functions are eigenfunctions of the linearized equations and each pair of such functions for temperature and velocity we call a mode of the system. For every wave number there are two possible eigenmodes, one with temperature and vertical velocity in phase and one with them out of phase. Only the in-phase motions are unstable in the sense that they derive energy from the buoyancy and viscosity and owe their existence, if any, to non-linear interactions. In the general turbulent situation, when we make a representation of the velocity and temperature fields, we must include both kinds of mode. In general then, the correlation between



vertical velocity and temperature is not unity, but some smaller value which depends on the relative amplitudes of the two kinds of modes.

We have then a mechanism by which the turbulence can lose its energy, besides that of the cascade processes contained in the ideas of KOLMOGOROV and HEISENBERG. Motion can be induced in a large scale by the buoyancy force; this motion will have temperature and vertical velocity in phase. But the non-linear term can then act to convert this motion in part to one with an arbitrary phase—we would consider this a mixture of in-phase and out-of-phase modes. That part of the motion in out-of-phase modes is then damped by buoyancy force and lost back into gravitational potential energy. Whether this mechanism of randomizing the phases is more important dynamically than the usual cascade process for setting the form of the power spectra depends on the parameters of the system. So far we have considered the low Prandtl number case where randomizing of phases is not important, but we plan to go on to consider more general situations.

This must suffice as summary of the physics of our approach, since time does not permit a discussion of the details. In a qualitative way the results agree with those discussed by MALKUS, but there is one significant difference. We do not get a cut-off in the heat transport spectrum.

In MALKUS' paper there is an explicit assumption that the smallest scale of motion is the marginally-stable motion on the mean field. Our results on this give the classical power spectrum of turbulence for the velocities as a function of  $k$ . There is  $k_0$ , which is the largest permitted scale; since you have a finite system, you cannot have wave numbers going down to zero for the motions satisfying the boundary conditions.  $k_0$  is the smallest wave number that can occur. And so we get a spectrum which starts as  $k^{-7}$ , but very quickly makes a transition to the  $k^{-5}$  law; ultimately, in the dissipation region, it goes as the familiar  $k^{-7}$  law. The spectrum for temperature fluctuations, in this low Prandtl number case, drops off initially like  $k^{-11}$ . At  $k = 2k_0$  the value of the temperature spectrum is quite low but the velocity spectrum has appreciable amplitude. This result seems relevant to the solar photosphere where it seems that the granulation, which measures temperature fluctuation, does not go down to the small scale on which we expect to find velocity fluctuations.

Let me turn now to the problem of convection in stars. Mrs. BÖHM's talk showed that we need to add something to our present models of stellar convection zones; in her theory it would be a value for the mixing length. The possibilities available at present seem to be to try to apply either Malkus' approach or the one I have just discussed. In either case we have to determine the appropriate complete set of functions to use—in the Princeton scheme these would be the normal modes of the linearized equations of motion. We also need to know the growth rates for each mode; that is, if the time-depen-

dence is like  $\exp[nt]$ , we want to find  $n(k)$  for all  $k$ . From the classical case, *i.e.* that studied by RAYLEIGH, we have the following simple picture:

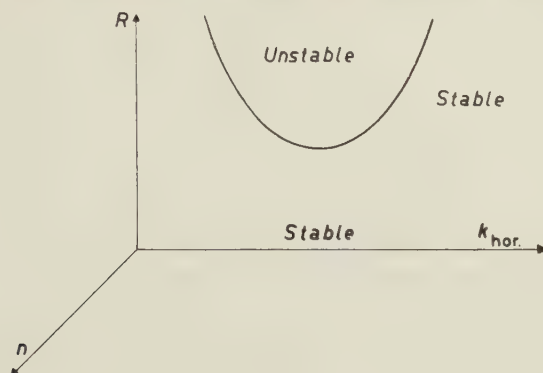


Fig. 2.

In the  $R$ - $k$  plane we have shown the critical curve for instability of a fundamental mode. The growth rate,  $n$ , for unstable modes is given by a surface which is above the plane inside the critical curve and below outside. The suggestion then is to find this surface for atmospheres varying in density from top to bottom and bounded by stable layers of gas. Much work by many astrophysicists has gone into this determination, but it is far from complete. Time does not permit the discussion of that work. Let me close by saying that it is our hope to couple such work with the non-linear procedure I have outlined above.

— G. K. BATCHELOR:

Sometimes it helps to have a greatly simplified view of a problem. I want to say a few words about what I think is the essence of the calculation of SPIEGEL's. My remarks concern the situation which I understand is relevant for stellar atmospheres—namely, the situation in which the Prandtl number is very small because the conductivity includes the effect of radiation and is very large—very much larger than the kinematic viscosity. We wish to obtain information about the mean-square temperature fluctuations and mean-square velocity fluctuations, for given values of temperature gradient or temperature drop or something of the kind. For those simple averaged quantities one can give a physical argument which I think is essentially right despite the very simplified character. The level surfaces of mean temperature are horizontal planes, and temperature fluctuations are produced by convection carrying these surfaces upwards or downwards against the action of conduction (which includes radiation), which would tend to keep them horizontal. Since the

Prandtl number is small compared with one, and the Reynolds number is large compared with one, the motions are uninhibited and take place freely and actively, whereas the temperature fluctuations tend to be suppressed. So on the whole, there is not much departure from the situation in which the level surfaces of temperature are horizontal planes despite the fact that there are big up and down motions. That general picture enables one to extract from the equation of motion and the equation for the temperature field estimates of the general magnitude of the velocity fluctuations and the temperature fluctuations. First of all, for the equation of motion, one would have as representing the order of magnitude of the acceleration  $V^2/L$ , where  $L$  is the scale of the motion of the energy-containing eddies. This will be balanced by—or supplied by if you like—buoyancy forces. Viscous forces we may neglect in view of the small value of the viscosity. Buoyancy forces will have as their general order of magnitude  $g(\alpha\delta T/T)$ . Then there will also be an equation for the temperature,  $T$ , which is just the heat conduction equation with allowance for the effect of convection. The total derivative of the temperature can be represented by  $V\beta$ , where  $\beta$  is the vertical gradient of mean temperature, fluctuations in temperature gradient being negligible, since temperature fluctuations are inhibited by conductivity,

$$\frac{V^2}{L} \sim \frac{g\alpha\delta T}{T},$$

$$\beta V \sim \frac{K\delta T}{L^2}.$$

Here one has a pair of equations from which to determine  $V$  and  $\delta T$  in terms of  $L$  and  $\beta$ :

$$V \sim \frac{\alpha g \beta}{K T} L^3, \quad \delta T \sim \frac{\alpha g \beta^2}{K^2 T} L^5.$$

This I think gives the general order of magnitude of these quantities  $\delta T$  and  $V$  representing the root-mean-square temperature fluctuation once one knows, by observation or any other means, the scale of the whole motion and the temperature gradient against which the motion takes place.

— E. BÖHM-VITENSE:

This is really a remark answering the question of BIERMANN whether, according to the investigation of MALKUS, we would expect the numerical results to be changed by more than a factor of 2 or 3. Now the theory on which our calculations were based was criticized very hard yesterday. It seems the main criticism was against the philosophy on which the theory was based, rather

than against the numerical values that have been obtained—at least this was the impression I got from various discussions. Now, I am not interested in the philosophy—what we need are numbers and so I shall just talk about the numbers. If we can expect that this principal of maximum energy flux, which was derived by MALKUS, applies also to stellar atmospheres—I think then we shall derive numerically results that are in good agreement with the ones we obtain; because, we just chose our scale length of the motions in the way that we got a maximum energy flux. If we correlate the scale length to the wavelengths with maximum instability, according to the calculations of BÖHM and RICHTER, we find, within the limits of errors, of course, the same length that was introduced into the calculations to obtain the numerical results as I pointed out yesterday; wavelengths of maximum instability can be estimated to be larger than 300 km and the length used was of the order of twice this length. So, I think the numerical results can still be expected to be correct within a factor 2 or 3.

— E. SPIEGEL:

A factor 2 or 3 in what—the temperature or the energy transport?

— E. BÖHM-VITENSE:

I was thinking about the convective energy flux in the critical layers, that means in the upper layers of the convection zone.

— A. J. DEUTSCH:

Do I understand correctly that these considerations all refer to the regions not observed, in terms of velocity fluctuations? The part of the atmosphere where the absorption lines are formed is the radiatively stable part, but there were suggestions in Spiegel's comments that the tail of the curve may indeed be responsible for what we have called micro-turbulence. Is there a reason to expect that perhaps it is just these small eddies that will be observed in the spectral lines?

— E. BÖHM-VITENSE:

I would say that not just the tail of the spectrum would overshoot into the stable region. I would think that also the main wave numbers could easily overshoot. If there is anything true to the picture that the material is moving with a certain velocity because of the buoyancy force, then this material will rise to the upper boundary of the unstable layer with a certain surplus temperature—so there is no way to stop them; they will just go on up.



— E. SPIEGEL:

What I was really only trying to say was, that there is a micro-turbulence in the velocity field, which we think provides the energy for the acoustic noise generation.

— A. UNDERHILL:

Is it then a reasonable generalization of what has been said to state that your calculations indicate that irregular velocity fields are carried upward across the border of the convective zone, but that very few temperature inhomogeneities are carried across by these turbulent motions?

— E. SPIEGEL:

Let me be more specific about these non-linear terms. In the dynamic equation, they are the  $U \cdot \Delta U$  terms, and their size relative to the viscous terms is the Reynold's number. For a large Reynold's number, as in stellar atmospheres, we expect a large range in the scales of motion because of relatively small viscous damping. In the energy equation, the non-linear terms are the  $U \cdot \Delta T$  terms, and their size relative to the heat-conduction terms is called the Peclet number. For small Peclet numbers—such as occur in stellar atmospheres because of the great efficiency of radiative conductivity—there is relatively large thermal damping so that the amplitude of small-scale temperature fluctuations is relatively small. Therefore we might expect fairly large amounts of kinetic energy in small scales, even though there would not be appreciably large fluctuations in temperature on the same scale. In particular, the heat transfer due to convective motions would be small, so that you could indeed have velocities without any effects whatsoever showing up in the thermal structure. Radiative equilibrium for model-atmosphere theory for hot stars could be quite good and still there would be fairly reasonable velocity fluctuations. I would like to suggest this in answer to the question raised by a number of people; I remember DE JAGER raised it—namely, where do the observed motions in the hot stars, in the early stars, arise? The motions could arise in convectively unstable zones in those stars in which the instability is due to the first or second stage of helium. They could be those motions in which velocity fluctuations are appreciable, but which would not show up in our model-atmosphere calculations. And I think it is the answer to Miss Underhill's question.

The question of DEUTSCH was how did the remarks I was making in regard to the application of these processes to the variable density atmosphere relate with what we might expect to see in microturbulence? In the scheme I pro-

posed, we first look at the linear equations for the variable density atmosphere and calculate the eigenfunctions to use as our complete set in which to discuss the turbulence. This is convenient because of the simple form the equations take when you use the right set of modes. These modes have the general property that they are decaying exponentially in the stable layers, they get large amplitudes in the transitions, and then it turns out they drop off exponentially below. The drop-off distance is  $1/k_h$ , where  $k_h$  is, in fact, the horizontal wave number of the motion. The most relevant modes, the ones that would do the most convecting, are those that are the most unstable in the sense that I defined earlier. I need not redefine it, I think the term is suggestive enough. And the most unstable ones, one has at the moment from the calculations, appear to be those for which  $k_h$  is on the order of the scale height near the top. Therefore, it turns out that the most dominant modes of the spectrum are those modes that are also related to the scale height. But, as pointed out earlier by WHITNEY, the scale height is also of the order of the free mean path of a photon. Therefore, already the smaller scales of motion which will be generated, if they exist at all appreciably, will be by definition microturbulence since they are on a scale which is smaller than the mean free path of the photons. Only the largest modes could be seen as visibly excited. Moreover, it is a suspicion that the temperature scale would drop off steeply and that, therefore, one would see effects in the temperature field only from these largest modes but that there would be an appreciable tail of velocity spectrum which would contribute to the observed micro-turbulence through the line-profile. The point being, as Mrs. BÖHM stressed, the largest scales will be the most efficient in getting up to the stable region. They will have the strongest amplitude in this region. So that, therefore, these will be the ones that will be most related to the observed granules.

— A. UNSÖLD:

Let me give an astrophysicist's summary. First, note that we have dealt so far, explicitly, mostly with problems involving two boundaries. What I have learned this morning from the talks by MALKUS and especially SPIEGEL, is that, having a limited atmosphere heated from below, then we get a distribution of the energy over different wave numbers  $k$ . We begin with one smallest  $k$ , the scale being determined by the thickness of the atmosphere. Then, SPIEGEL said, we get a fairly steep slope extending over a fairly small range of wave numbers, less than a factor of 2. These modes are evidently driven directly by putting in thermal energy from below. The following modes with larger  $k$  are driven mechanically by the modes with greatest wavelengths, and follow essentially the well-known Kolmogoroff-Heisenberg theory of isotropic turbulence with a slope proportional to  $k^{-5/3}$ . Finally, when viscosity

becomes predominant, one obtains, a steep slope proportional to  $k^{-7}$ . So what is new to us is essentially what happens at the small  $k$ ; what follows at larger  $k$ 's is essentially driven by the longest waves. And we have learned that for the heat transfer, only this range of small wave numbers is important, which is rather obvious. Trying to link up these considerations with what we have heard before in astrophysics—chiefly the lecture by Mrs. BÖHM-VITENSE—it seems to me first that the equations which BATCHELOR wrote down were not so different from the old-fashioned mixing-length theory of Prandtl. The latter essentially just uses an average over the group of «small  $k$ » which is responsible for the heat transfer. To these, «driven modes» one might attach more or less a Kolmogoroff-Heisenberg spectrum which is produced by the dynamical pressure only. Here, the big whirls are divided up into smaller whirls and so on—purely by means of the  $(U \cdot \nabla)U$  terms in the hydrodynamical equations. It seems to me that the heat, which is finally produced through viscosity, is astrophysically in general not very important as long as we have velocities considerably below the velocity of sound, that is,—astrophysically speaking—as long as we are in the photosphere and not in the chromosphere.

The next point is: «What do we need more in astrophysics?» Summarizing a small colloquium which SPIEGEL, BIERMANN, and MALKUS and I had this afternoon, the chief point seems to me that we should find out how to pass from the problem with two rigid walls (mathematically simple because one can easily use the methods of Fourier analysis) to an atmosphere in which the scale-height as well as the degree of instability varies considerably with height. SPIEGEL correctly remarked that here the methods of Fourier analysis cannot be applied any more because one has no scale to begin with. Should it not be possible to find out as a function of scale-height and degree of instability, with a certain approximation, something like a variable characteristic length? Following that, as a function of depth, one might visualize on which scale the modes doing the heat exchange, and the KOLMOGOROFF tail attached to them (only of secondary importance in astrophysics) go on. *I.e.*, one should try to find an approximation where the fundamental wavelength or the fundamental  $k_0$  becomes a function of depth. That would, of course, not be a Fourier analysis in the strict sense, but something like an approximation familiar in optics where one also takes the wavelength as a function of the variable refractive index and follows a wave along a curved ray using within small intervals a plane wave solution. And the aerodynamical problem would be now to find some similar methods of attack considering the fundamental wavelength as a function of depth. Some people have considered whether this wavelength might be connected with the most unstable wavelengths; but that doesn't seem to find general acceptance. And so I should like to ask the hydrodynamics people whether our problem might be approached as kind of adaptable  $k$ -wall problem?

Then, finally we understand for the later type stars to a certain extent how the whole mechanics is driven thermodynamically and how the energy is transferred. For the hot supergiants, where we observe large motions, I think, we should not go too hastily over the problem. We have there very large motions; no doubt about the observations. But I think it is an open question, at least at present—what is the driving machine? SPIEGEL indicated his idea that it might perhaps be the helium convective zone. But recent calculations by Mrs. BÖHM about convection in hot stars indicate that in these hot stars the radiative energy transfer is so predominant that no turbulence element can keep any appreciable temperature difference. And so, the mechanism of an ionization convection zone doesn't work simply because all the « valves of the engine » are out of order. Another suggestion which BIERMANN reported and which has been worked out to a certain extent by KIPPENHAHN in Munich, is that the motions in the atmospheres of hot supergiants might be connected with their rotation. A rotating star must, in connection with the nuclear energy generation have meridional currents; and these in turn would lead to rather high velocities in the atmosphere.

— H. LIEPMANN:

I would like to point out here that if the mixing-length approach is used in ordinary compressible boundary layer theory one obtains 21 different theoretical results—all different. Also, I am always a little shaken with the astrophysical applications of theories of incompressible turbulence, *i.e.*, neglecting coupling with the sound waves and coupling with magnetic fields.

— R. N. THOMAS:

It seems to me this last is just the point. I thought you would comment on only the Kolmogoroff-Heisenberg interaction having been admitted, rather than also the compressibility dissipation, which UNSÖLD believes negligible.

— A. UNSÖLD:

No, I didn't talk about sound waves because they become important only if you approach the velocity of sound. Such motions are unimportant in the photosphere—and I propose to deal only with that—but are predominant in the higher chromosphere.

— R. N. THOMAS:

Forget the chromosphere-photosphere division and concentrate on the aerodynamics. My reference was to the coupling, through the non-linear terms



$U \cdot \nabla U$ , of the « eddy-turbulence » and « random noise » in the sense of the discussion by MOYAL and by UBEROI a few years ago, to which CLAUSER has implicitly referred in his general remarks in the Part I discussion. It was always my impression that compressibility effects could not be neglected when the Mach number exceeded about  $\frac{1}{4}$ ; note that it is  $\frac{1}{4} \div \frac{1}{3}$  in these lower solar photospheric regions you are discussing. If you want to be more general, and associate the observed « microturbulence » in other stars with the tail of the curve discussed by SPIEGEL, then I recall from Underhill's data that velocities run up to Mach  $0.5 \div 0.8$ , and in certain cases through Mach 1. So how can I confine attention only to the Kolmogoroff-Heisenberg kind of interaction, and neglect the compressibility?

— E. SPIEGEL:

I think the important thing is the coupling to the pressure field—that is a kind of acoustic term. And that I have proposed to try to get around by choosing the right eigenmodes for the system which will allow for all the complexities of a pressure field of a compressible motion. But as for the non-linear coupling, I think that one can neglect it. The compressible flow produces a distorted pressure field which is a standing pressure field—and that has its effects on how the amplitude of the velocity distributes itself. This is not the same as the acoustic term generated through the non-linear term essentially. If you take it in the wave number space—the amplitude of the velocity field is still kept to the wave number. So you don't have compressibility generated through the non-linear term even though you may at least in the work term try to allow for pressure effects.

— H. LIEPMANN:

I get also slightly worried that the existence of the Kolmogoroff spectrum is here taken for granted in spite of the scarcity of convincing experimental evidence. I feel perfectly fine as long as you say we have large-scale motions that have a tail of isotropic turbulence; but to go into too much of the details is likely to be wrong.

— G. K. BATCHELOR:

I thought that recent observations made by people in British Columbia provided extreme agreement with the  $k^{-5/3}$  law for the energy spectrum.

— F. H. CLAUSER:

The other day we were told that in this region in which the convection takes place, it was very important to get the right answer, otherwise big errors could

be made in stellar structure. I thought that as a result the heat transfer across that region was a sensitive thing; and now people say factors of 2 or 3 make no difference at all. I am lost.

— E. BÖHM-VITENSE:

We have done the calculation for  $l = H$  and then we have done the same calculation for  $l = 2H$ , to see the effect. And now in the layer where we have reached the adiabatic gradient already, let us say for pressure  $\log p_0 = 8$ , we find that the temperature varies from  $\log T = 4.45$ , in the one case, to 4.57 in the other case. So those are the size differences which occur.

— J.-C. PECKER:

But the differences that occur in the mass of the star, due to different values of the ratio  $l/H$ , when you carry the integration inward, are very large—the change in radius is also very clear.

— E. BÖHM-VITENSE:

This may well be—I just should draw your attention to one calculation that was done by SCHWARZSCHILD, who checked which characteristic length one should take in order to obtain the observed position of the sun in the H-R diagram. He found that if you do the kind of calculation we have done, you get the right answer for the characteristic length  $l = \frac{3}{2}H$ . This, of course, does not mean anything with respect to the method, but it does show that with these calculations you can at least get agreement with the observations.

— A. J. DEUTSCH:

At the Liège meeting last year, a paper was presented by TEMESVARY, who considered the effects upon red giant models of changes in the ratio  $l/H$ . The effect is sometimes enormous; at a given temperature, it can alter the luminosity of the star by a factor of 50. These results threw grave doubts on many of the conclusions that have been drawn from H-R diagrams about the abundances of metals in old stars. In some contexts, therefore, I think it must be very important to know the effect of these factors.

— A. UNDERHILL:

The point is, that in some spectral types the convective zone has an effect only on the upper atmosphere; but in others it affects the structure of the star as a whole. Particularly with cool stars, the question of convection is critical

for the internal structure of the whole star. The atmosphere convection has a different effect from the internal convection which has a major effect upon the size of the star. This is the point. As I remember it, the statement by Mrs. BÖHM referred to the upper atmosphere convection. Its effects are not so great because we are really saying nothing about energy generation. We are not disturbing the rate of energy generation, we are merely modifying the manner in which energy can escape from the star.

— L. BIERMANN:

The paper at the Liège meeting mentioned by DEUTSCH was the one where our group had explored the sensitivity of the solutions that you get for the evolution curve—as a function of the parameter expressing the ratio between the mixing length and the scale-height. We found first that disregarding the convection altogether does not lead anywhere; second, when you use mixing-length theory, then one finds for this particular kind of problem—for the stars which have moved away from the main sequence by increasing their radius by a factor of about 10 or 20—that taking for the said parameter the value one, or two, respectively, makes a considerable difference in the solution. This difference is so large that indeed (as was pointed out) it is impossible to derive, by comparison with the observed color magnitude diagram for instance, reliable values for the chemical composition which also enters there quite sensitively.

— W. V. R. MALKUS:

In the last session I mentioned an experiment in progress to study the effects of the penetration phenomena. The observations indicated very sharp limits on the convective motions, a limit which was well above the point of the maximum density. In other words, there was definite penetration into the stable layer. I believe we can analyse aspects of this problem, and I do not think they lie completely outside the scope of the analysis presented this morning. Perhaps we can get results that will be valid in those regions of the star which are inaccessible to us, experimentally.

I have two suspicions about this work we see outlined on the board. One notes that the whole spectral structure of the tail has been put in by assertion; and though the assertion may be sound in astrophysical settings, it is not possible to test it in the laboratory. To speculate further on the untestable hypotheses I think is unwarranted. LEDOUX and SCHWARZSCHILD have the intuition that the hypothesis is sound, but their confidence is based on the assumption that the fluid can find no other way than this cascade mechanism to dissipate its energy. However, I think SPIEGEL mentioned, and is now exploring, the

possibility that the energy available for release at a particular wave number can be either dissipated by cascading down the wave number spectrum, or it is possible for the motions and temperature fields to get out of phase at this wave number and prevent energy release. That is,  $U$  and  $T$  can be large and not exactly correlated. Hence, this large amount of dissipation is not required. All the observations that are made in the laboratory, and in the atmosphere where very large-scale convection processes occur, indicate that the correlation between velocity and temperature is quite small, even in those scales of motion which are the most responsible for the transfer of heat. This indicates phase blockage as we call it, inhibiting the release of energy from the mean field; and I offer this thought as caution in accepting the Heisenberg-like dissipation mechanism.

— C. DE JAEGER:

My first remark concerns temperature fluctuations and the inhomogeneous photospheric model. I think that all the work done in previous years on the inhomogeneous photosphere has to be revised and that nothing can be stated actually on the values of the temperature fluctuations as derived from line-profiles. We should remember that our opinions on the structure of the solar photosphere have considerably changed, especially those on the outer layers. Compared to previous results we know that the temperature in the outer part of the sun is not so low as BÖHM suggested in 1953; the temperature may be somewhere between 4000 to 4500, and we know that the temperature increases toward the chromosphere already near  $\tau = 0.01$  or 0.001. If further Lab's measurements of the continuous solar radiation are correct, the temperature in the whole solar photosphere has to be somewhat increased. Turning now to the problem of micro- and macroturbulence which has been discussed several times in the course of the meeting, I would make a short remark. In the stellar photospheres, we are dealing with a velocity field. The energy of this velocity field has a certain distribution with wavelength and we do not know what the distribution function is—it may have one peak or various peaks; it may even be just one frequency that is active. Now astronomers have in effect developed two methods for studying this velocity field. These methods effectively consist in filtering out a certain part of the energy of the velocity field—one filter is called microturbulence and is effective only for wavelengths  $\lambda$  such that the product of the absorption coefficient  $\kappa$  ( $\text{cm}^{-1}$ ) and  $\lambda$  is small compared with unity. The other filter is called macroturbulence, it refers to the region where  $\kappa\lambda \gg 1$ . So we only get a part of the velocity spectrum, but we want to know all of it. If the situation is, as is suggested by the observations, that the main part of the energy of the field is fed into elements with characteristic lengths of the order of the scale height of the



atmosphere, then we are in a bad situation. We know that the relation between the absorption coefficient  $\kappa$  and the scale height  $H$  is in nearly all cases such that  $\kappa H$  is of the order of unity, both for the continuous and the line spectrum—so that the main energy is neither in the region  $\kappa\lambda \ll 1$  nor  $\kappa\lambda \gg 1$ . That means that especially when studying micro-turbulence we always get a rather small part of the true energy of the spectrum, and this may perhaps have some relation—I come to the next point that I wanted to discuss—to the anisotropy of the turbulence field. WADDELL and SUEMOTO have found that the turbulent velocity field might have a certain anisotropy; but it is not quite certain to me that there this anisotropy is really in the velocity. It might fairly well be that it is the scales that are not isotropic. So, when approaching the limb, the effective scale of our elements becomes larger and that might have for effect that we get a greater part of the turbulent spectrum through our «filter», than by looking straight inwards to the sun. We should bear in mind that in a free atmosphere with a density gradient anisotropic scales might well occur in the turbulent motion field. *E.g.*, it has been found in the high terrestrial atmosphere near the 100 km level that the vertical scale of the motions is of the order of the atmospheric scale height, 6 km, while the horizontal scale is of the order of 150 km. Of course, the terrestrial atmosphere is not directly comparable to a stellar one, but we should keep this case in mind.

— E. BÖHM-VITENSE:

I would like to ask DE JAGER—I do not quite understand your first point, because in the line-profile investigations, you always include small scale *and* large scale motions; so if you find a change in line-profiles, you could not explain it as due only to the transfer of the velocity field from macro- to micro-turbulence.

— C. DE JAGER:

Of course, the whole velocity spectrum contributes to the detailed line-profile, but in my feeling nobody has worked out a reliable way to determine the *detailed spectrum* from the line-profile (which is widened by so many different and badly known causes). The only direct ways to study the motion field are either by considering the position of the flat part of the curve of growth («micro-turbulence»,  $\kappa\lambda \ll 1$ ) or by considering the line widening which does not influence the equivalent width («macro-turbulence»,  $\kappa\lambda \gg 1$ ).

## PART IV.

# Considerations on Localized Velocity Fields in Stellar Atmospheres: Prototype — The Solar Atmosphere.

## C. - Transient Velocity Fields in the Lower Solar Atmosphere.

### Summary-Introduction.

A. B. SEVERNY

*Crimean Astrophysical Observatory - Pochtovoje*

#### 1. - Introduction.

Speaking on localized velocity fields on the Sun, we mean the velocity fields in active regions on the disk. The experimental data used to picture these fields are based mainly on 1) the observations of Doppler shifts of spectral lines, 2) the form (asymmetry) of line profiles, 3) moving picture process in monochromatic, mainly  $H_\alpha$  and  $H$  or  $K$ , light. However the big skin-time of solar plasma makes these data insufficient to get an adequate and complete idea about the motions in active regions, and we must also take into account the available observational data about magnetic fields in these regions. Somewhere the state of affairs permits one to disregard the possible influence of magnetic fields on the picture of velocity fields ( $H^2/8\pi < nkT$  or  $\frac{1}{2}qv^2$ ). But these occasions are comparatively rare, because the main source of solar activity is closely, but in a not quite recognized way, connected with magnetic activity of the Sun. This is why we should in our talk consider as closely as possible both subjects—the motions in active regions and their magnetic fields. The best illustrations of the necessity of such a mode of consideration are the velocity fields in sunspots.

#### 2. - The velocity fields in sunspots.

Peculiar motions in sunspots were discovered by EVERSHED (1909, 1910) and were considered as in-streaming of gases towards the axis of the spot in outer layers and out-streaming in deep layers. ABETTI (1932, 1934) found an azimuthal component in this motion and indicated a possibility of spiral-like motions. The vortex structure around spots, as seen on  $H_\alpha$ -spectroheliograms, induced the idea about vortex motion around sunspots (HALE, 1908; BJERKNES, 1926) like Earth cyclones and this can easily be supported by a simple

consideration of the action of 'Coriolis' force on the Evershed motions. However we don't have as yet direct observational evidence of such vortex motions; and the vortex structure, which is unique rather than typical, is considered as a manifestation of the peculiar character, of the magnetic field around spots.

The observations at McMath observatory (McMATH, PIERCE, MOHLER, GOLDBERG, 1956) and the extensive and detailed investigations of the Evershed effect carried with the aid of the Crimean Solar tower by BUMBA (1959, 1960) revealed the fine spectral structure of this phenomenon. It appears to be not a kink of a line but a broadening as a whole, with strongly pronounced asymmetry of opposite directions at the opposite borders of penumbrae. At perfect conditions of seeing, this asymmetry is simply a jet-like component—« flag » is similar to that produced by a spicule, and instead of « flags » we observe sometimes a separate component—a secondary faint satellite of the line. At bad seeing and low resolving power this phenomenon of « flags » disappears, and we observe the usual Evershed pattern, *i.e.* the kink of the line (\*). The maximal velocity measured in « flags » reaches  $(7 \div 8)$  km/s, and the tilt of velocity vector to the solar surface increases with increasing depth (the Michard model was used to determine the effective depths of different lines). If the plasma is able to move across the lines of force with such velocities (despite the fact that  $H^2/8\pi > \frac{1}{2} \rho v^2$ ) the whole magnetic field of the sunspot can be disintegrated during  $10^9 \text{ cm} / 8 \cdot 10^5 \simeq 10^3 \text{ s}$ , which time is too short for the life-time of a sunspot. This strongly suggests that these motions proceed along the lines

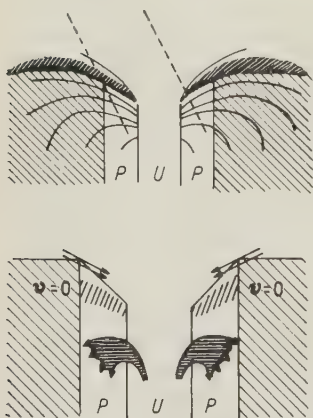


Fig. 1. — The pictures of magnetic field (above) and motions (below) in spot (*P*: penumbrae, *U*: umbrae).

of force of the magnetic field forming something like a « fan ». The following picture of motions resembling the behaviour of integral curves near a saddle point seems to be the most probable one (Fig. 1), and the most interesting is the change of the direction of motions near the boundary between the photosphere and chromosphere and the growth of velocity from this boundary downwards and upwards.

The fact of opposite directed motions (in-streaming) in the outer layer (chromosphere) of the sunspot has also been checked directly by the author, who used the magnetograph to record the magnetic fields and line-of-sight velocities simultaneously in the chromosphere *above the limb* for the spot immediately near the limb (Fig. 2). From the run of line-of-sight velocity and radial component of the magnetic field one can easily draw

(\*) This effect is most pronounced, as is well known, at the border of the Sun, when the slit is set along the radius.

the conclusion that the change of magnetic polarity is accompanied by the change of radial velocity as if the flow of plasma proceeds along the lines of the magnetic field of the sunspot (SEVERNY, 1960*b*).

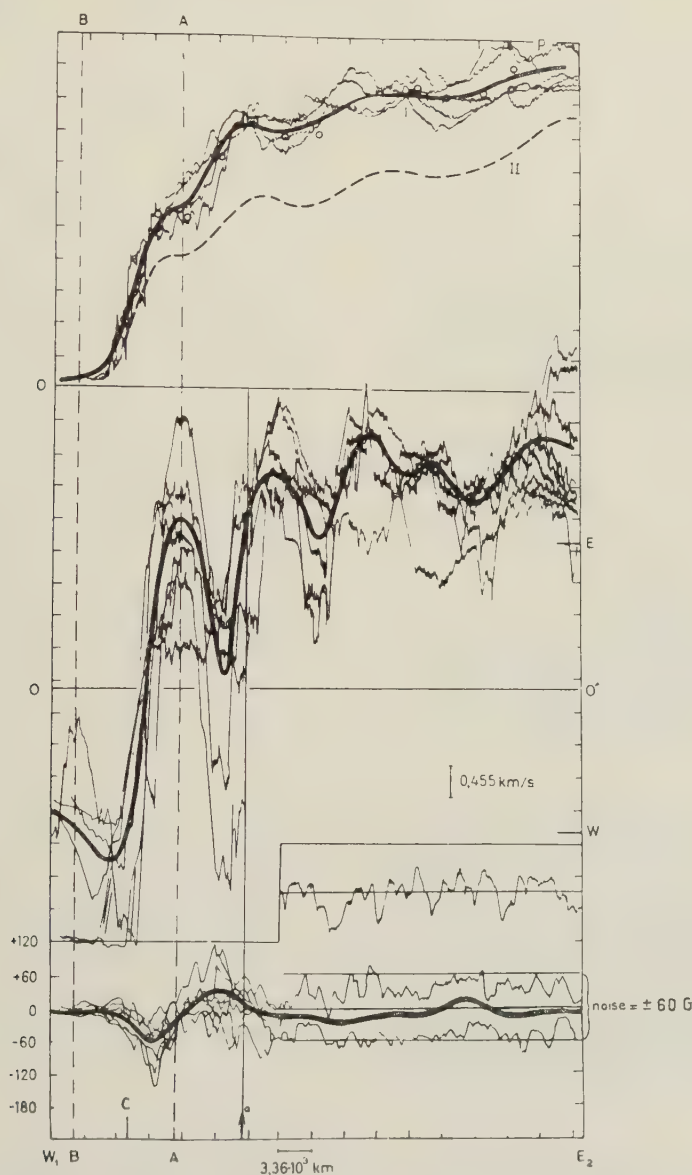


Fig. 2. - The records in  $H_\beta$  of brightness (above), radial velocities (in the middle) and magnetic field (below) for the spot July 24, 1959 near limb (dashed vertical line A.A). Arrow  $a$  and full vertical line correspond to the spot; BB: the top of hydrogenic chromosphere;  $OO'$ : corresponds to zero velocity on the Sun. Vertical scale of the field in gauss.



These conclusions are mainly based on the observations outside umbrae. However the observations of CHEVALIER (1913) and THISSEN (1950) indicated the possible existence of fine structure in umbrae—a kind of granulation. Our attempt to examine with the aid of the high resolving power magnetograph ( $2'' \times 2''$ ) the fine structure of the umbrae showed clearly the existence of separate condensations of field lines-of-force inside umbrae, the condensations with characteristic size of several seconds of arc (see Fig. 3). But the most

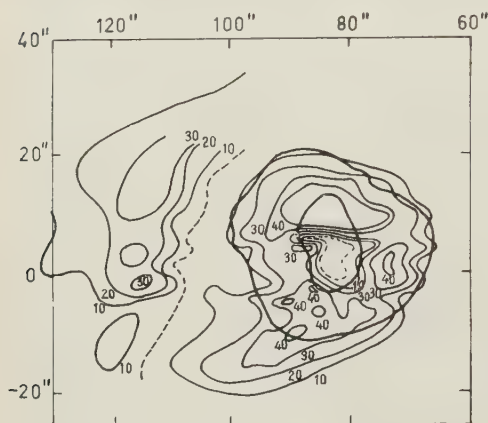


Fig. 3. — The map of isogauss for big spot Sept. 26, 1958 near the center of the Sun. The unit is 15 G. Dashed closed line is neutral line. Heavy full line is contour of umbrae and penumbrae.

striking phenomenon is the appearance of a pure *transversal* field in the middle of the umbrae of the big spot near the center of the disk, observed in perfect conditions of seeing. This fact may be considered as evidence that convection still exist in the very umbrae (private communication of Prof. COWLING) and the spreading of the ascending current dragging the frozen magnetic field with it in horizontal plane produces the effect of transversal field. But the appearance of Zeeman triplet in umbrae may as well also be connected with the depolarization effect due to fluctuations of magnetic field with depth or due to depolarization by collisions (SEVERNY, 1959).

In any event the velocity field in the sunspot must be closely connected with the structure of the magnetic field ( $H^2/8\pi > \frac{1}{2}\rho v^2$  for the most part of sunspot plasma) and this field plays a primary role in all phenomena over here. It seems improbable that this field is pure longitudinal because of instability of such field—it must expand. We may consider the observed fine structure of the field in umbrae as a manifestation of such instability of pure longitudinal field. We think the force-free field or similar to that is the most probable configuration of a stable magnetic field of sunspots and for securing the long life of sunspots. This conception offers also the possibility to explain the observed vortex-cyclonic structure around sunspots and spiral-like motion inside the spots.

### 3. — The motions of flares.

At first flares were considered as stationary formations (ELLISON 1949). First evidence of motions in flares was obtained for flares of May 15-16, 1951 at McMath (DODSON and McMATH, 1952) and Crimean Observatories (SEVERNY

1952b). The expansion of flare area accompanying the growth of  $H_{\alpha}$ -intensity might be considered as a spreading of excitation. However the spectroscopic observations of line-of-sight velocities in flares indicate real motions of neutral hydrogen in flare regions. Let us consider first the data from the moving picture technique in  $H_{\alpha}$ -light.

On the disk, except for expansion of the flare region, about 30% of flares show the rapid motions of jets or plasmons (SEVERNY and SHAPOSHNIKOVA, 1952, BRAY, LOUGHEAD, BURGESS and MCCABE, 1957) with supersonic velocities. Sometimes in great flares at their further development, we can observe the motions of luminous fronts or shells moving with supersonic velocities  $\sim 300$  km/s and the bending of these fronts when they approach sunspots. The course of events is nearly the same as if some shock-wave were partly reflected by the magnetic field of the sunspot. There is a similarity in the motion of these fronts and shock fronts; namely, the thickness of the luminous front (or intermediate layer) decreases with increasing velocity according to approximately the same dependence in both cases (GOPASUK, 1958).

However the most important data may be obtained from the observations of flares above the limb. More than 25 limb-flares from 180 observed with the Crimean coronagraph have been considered in detail; and for 14 flares the motions, brightness and area were measured (SEVERNY and SHAPOSHNIKOVA, 1960).

It was found that a great majority of flares above the limb appear as a brilliant hill (with sharp conical edge, Fig. 4), the upper front of which undergoes a rapid dilatation and after that a contraction with velocities from 50 km/s to 600 km/s. These dilatations are not uniform but show pulsations. The corresponding accelerations are very high ( $5 \cdot 10^4$  to  $10^6$  cm/s<sup>2</sup>). The most characteristic is a «cumulative» effect on the upper edge of the flare (the for-



Fig. 4. — The typical limb flares.

mation of a conelike top) and its successive contraction excluding the conception of the flare as a simple explosionlike phenomenon. (It is interesting to note that the rate with time of the distance of the upper edge from the original «core» of the flare is considerably higher than that observed at nuclear

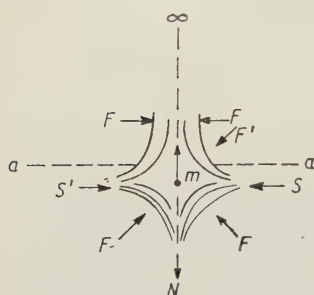


Fig. 5. — The cusped field geometry of magnetic field to explain the motions in limb flares;  $aa$ : solar surface;  $N$ ,  $S$ ,  $S'$ : magnetic poles;  $F$ : electromagnetic force acting on plasma  $m$ .

explosions (TYLOR, 1950)). The cumulativity of explosionlike flare-prominences can be explained by the peculiar geometry of the crossed magnetic field (cusped-field geometry) surrounding the flare, which appears in neutral point of this field due to the pinch-effect. A high-temperature plasma of flare trapped in this case in a «magnetic bottle» tends to expand in the direction of least counteraction from the surrounding field (on Fig. 5 shown by arrow). The electrodynamic acceleration of a current appearing in the neutral point can attain observed values at the strengths of 100 G of surrounding field.

Therefore moving-picture records show that in flares we deal with the formation of *cumulative jets* of plasma (or plasmons), moving with supersonic velocities, and exerting a supergravitational acceleration most probably under the action of electromagnetic fields. (Something like plasmotronic motions *e.g.* of the type realized by KOLB (1958) and ARZIMOVICH, LUKJANOV, PODGORNYY and CHUVATIN (1957)).

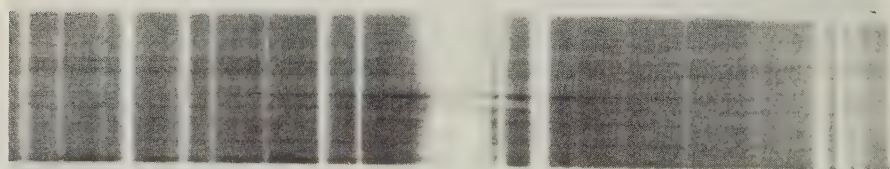


Fig. 6. — The «Moustaches» in  $H_{\alpha}$ .

Turning to the spectroscopic observations we should at first consider the phenomenon of fine structure of flare emission, or of moustaches, out of which the extremely extensive wings of  $H_{\alpha}$ -lines in great flares are composed (SEVERNY, 1957 *a* and *b*). Besides, the phenomenon of moustaches has its own importance in the problem considered. The spectroscopic investigation of moustaches (Fig. 6) showed that the far wings of lines originating in the very

« core » of a flare, may be considered as broadened owing to the Doppler effect of some kind of « macroturbulence » with velocities from 80 to 250 km·s<sup>-1</sup>. The most convincing evidence for this can be found in limb flares. If the emission of far wings is optically thin and broadened by Doppler effect, the velocities of atoms in this process can reach 1000 or more km/s.

In most of the cases, the blue wing of moustaches at the disk is brighter and broader than the red one, which may be considered as an evidence of ejection of atoms out of the grains of peculiar emission. This asymmetry does not depend on the position on the disk and indicates that the process of ejection is similar to explosion predominantly in two opposite directions, and not to a process of pure radial ejection. This is confirmed by the appearance of moustaches tilted to the plane of dispersion. The phenomenon appears at different levels in the solar atmosphere, and when it is found in deep layers the undisturbed absorption line is shifted to the violet indicating velocities  $\simeq 3$  km/s lifting up these grains of emission.



Fig. 7. — The typical emission in core of  $H_{\alpha}$ -line in flares.

All the above-mentioned spectroscopic phenomena relate to the outermost part of lines; *i.e.*, to the regions of high Doppler velocities. The inner part—the core of the line in flares—shows often an asymmetry; and the most typical form of this asymmetry is the predominance or excess of emission in the red side. The emission is stronger and more extensive over the disk in the red side of the line than in the blue side, however the blue part of the line is more extensive along the spectrum (see Fig. 7). This indicates the complex character of the motions—presumably the contraction and expansion (see below) of flare plasma. The contraction of the region of continuous emission in flares was also observed by ZIRIN (1959). These facts are compatible with moving picture data and can be explained by ideas about pinch-effect (see below).



Summarizing briefly our observational data about *magnetic fields* connected with flares, we can state the following (SEVERNY, 1960a).

1) Flares appear practically in *neutral points* of magnetic field of spot groups ( $H=0$ ,  $\nabla H > 0$ ). This

was observed in 54 cases out of 61; in 7 cases the discrepancy is bigger than errors.

2) The flare phenomena lead to *simplification* (or «destruction») of fields surrounding neutral points—the reducing of  $\nabla H$ , the vanishing of nearby magnetic hills, etc. This process was observed in 7 cases out of 8.

3) For six flares, besides that, GOPASUK has recently observed the *contraction* of configuration of distant magnetic poles after the flare.

See Fig. 8 illustrating these results.

These facts led us in 1957 to the idea about the *transformation of magnetic energy* into heat energy in flares. If the distribution of the field near neutral points looks like that presented on Fig. 9, the plasma contracts near the neutral point until two shocks approaching the neutral point appear. The collision of these shocks heats the plasma impulsively in a small region up to  $T \sim (10 \div 30) \cdot 10^6$  °K. This is a kind of pinch-effect.

One of the most suggestive observational data in favour of this point, beside the data

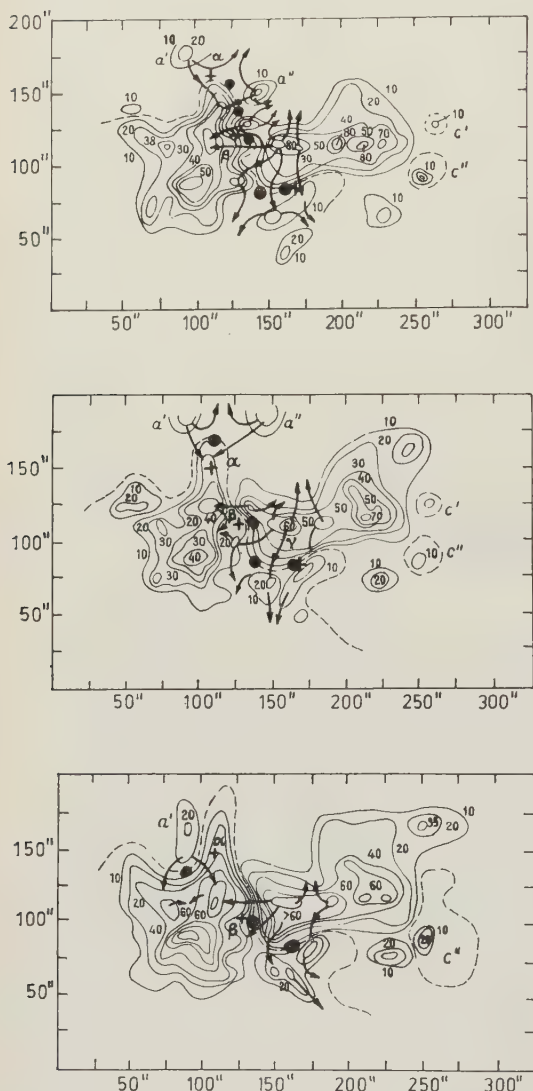


Fig. 8. — The successive maps of magnetic fields recorded in  $\lambda$  4886 before and after the flares of importance  $2^+$  and 3, Aug. 22, 1958 showing the simplification of field after flares. (Unit=10 G). Full dots: neutral points, crosses: locations of flares at their origin. Dashed line: neutral line. Arrows: approximate directions of field tubes.

mentioned above, is the records of line-of-sight velocities which show that neutral points of  $H$  are practically always (in 30 cases out of 37) found on the neutral lines of velocity maps—in places where two motions have opposite velocities and where they are often off the scale of the recorder (very steep change of velocities), see Fig. 10 showing maps—one of magnetic field and the other of radial velocities.

In case of two *distinct* layers approaching or escaping each other we should observe the splitting of a spectral line. But in the case of a contraction continuously distributed around the neutral point we will have predominance of  $+$  velocities from one side and  $-$  velocities from the other side of the neutral point. The undisturbed matter between two shocks is pushed away from the neutral point in both opposite directions perpendicular to the motion of shocks, and this presumably produces the observed phenomenon of moustaches and the steepest change of velocities near neutral points.

Pinch-effect around the neutral point in the free-field can be set up by the annihilation of azimuthal fields of approaching force tubes with opposite longitudinal fields (see for instance GOLD and HOYLE (1960)). The contraction of plasma in the region of the neutral point can also be produced by rapid

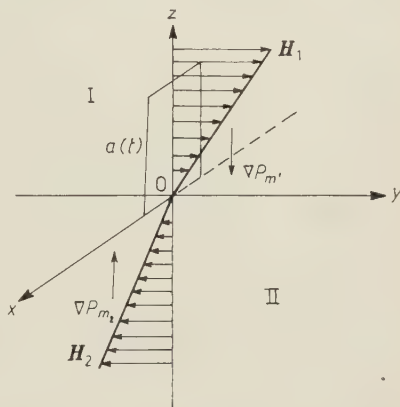


Fig. 9. — The distribution of field strength near neutral plane  $XOY$  leading to pinch-effect, (see SEVERNY, 1958).

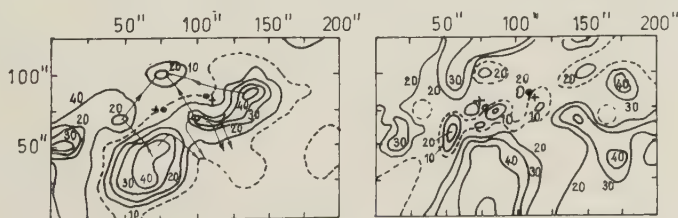


Fig. 10. — The maps of magnetic field (left) recorded in  $H_\beta$  and radial velocities (right) of the same active region. The notations are the same as on Fig. 8.

changes of sunspot fields if these fields are external, of dipole character. In both cases strong shock-waves converging to the neutral point can appear. It was shown that the incident shock front outruns the region of strong magnetic field. As a result of reflection of incident shocks in the neutral plane the stationary high temperature region is formed behind the front of the re-

flected shock moving in the contracting plasma towards the region of strong magnetic fields (SEVERNY and SHABANSKY, 1960). The magnetic energy is thus used up for heating this region and this heating may cause thermonuclear reactions in flares. The undisturbed plasma between approaching shocks will be pushed in two opposite directions perpendicular to the direction of shock motion. These considerations can explain some principal features of observed phenomena including the generation of cosmic rays (SEVERNY and SHABANSKY, 1960).

#### 4. - Flocculi and faculae.

BABCOCK was the first who found a connection between calcium flocculi and magnetic fields (BABCOCK, 1960). STEPANOV confirmed this result and examined velocity fields in flocculi with the following results (STEPANOV, 1958, 1960):

1) Stationary descending motions are observed over the 80% of the area occupied by flocculi and the corresponding mean velocities are

$$V_{\text{deac}} = +1.7 \text{ km/s},$$

$$V_{\text{asc}} = -1.0 \text{ km/s}.$$

It follows that the excess in flux  $S \times V$  is 4 times in descending motions as compared with the ascending one.

2) The observations of flocculi *near the limb* showed the *inflow* of gases into the region occupied by flocculi and this inflow is 10 times smaller than inflow through the upper boundary. This excess of inflow is probably compensated by outflow of matter during non-stationary processes such as flares and surges; and an approximate picture of the motions is shown in Fig. 11.

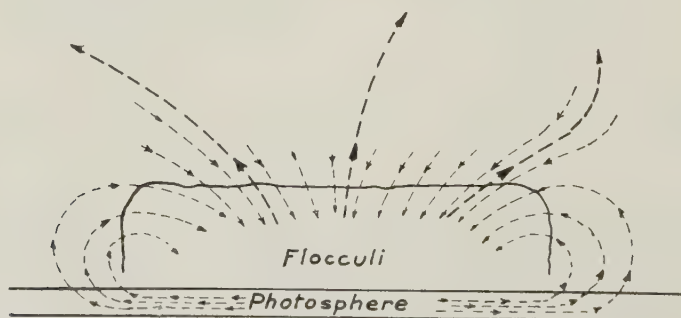


Fig. 11. - The approximate picture of motions in flocculi.

These relates to  $\text{Ca}^+$  chromosphere. In *deep layers* (photosphere) the picture depends on the strength of  $H$ ; and STEPANOV found that for strong  $H > H_0$ , the direction of motions follows closely the magnetic polarity as well as the line of zero velocity, which is similar in form and position to the neutral line of  $H$ . The example is in Fig. 12.

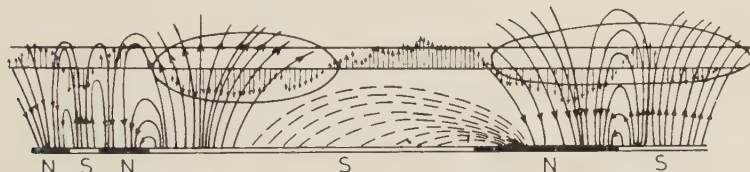


Fig. 12. - The correspondence between motions and magnetic fields (at considerable strength) in faculae.

If  $H$  is weak only descending motions are observed independently of polarity of magnetic fields.

These results indicate that the motion of plasma in flocculi and faculae is governed by the magnetic field only if this field is strong enough. In the opposite case we have in-streaming of gases into the region of flocculi.

To explain the connection between magnetic field and the active regions including faculae, PICKELNER has recently pointed out that the weak field increases the stability of convection streams and decreases the turbulent viscosity in the upper layer of the Unsöld zone. It should increase the convection velocity, which explains a small gradient of temperature in faculae, the increase of the flux of acoustic energy, and the heating of the chromosphere and the corona above active regions. The further increase of magnetic field may stop convection in the way described, for instance, by BIERMANN (1941).

## 5. - Prominences.

Here we have the most clearly defined motions of the solar plasma. The proposed classification of prominences according to the predominant type of motion in them (SEVERNY, 1952 and SEVERNY and KHOKHLOVA, 1953) seems to possess a general physical meaning, although it does not reflect all peculiarities of motions and behaviour of phenomena. The turbulent or irregular motions which are characteristic for quiescent prominences are developed, as a rule, far away from strong magnetic fields. The histogram of these motions (the number of knots *vs.* velocity) as compared with the two dimensional cross-section of a Maxwellian distribution shows considerable deviations of these irreg-



ular motions from the usually adopted picture of astronomical turbulence. DUBOV (1955) found this irregular behavior of quiescent prominences may be described in terms of locally-isotropic turbulence with a Kolmogorov spectrum.

In the presence of strong magnetic fields, *e.g.* that of sunspots, the predominant type of motion, on the contrary, is the «electromagnetic» one—the movements of knots and streamers proceed along curved paths nearly corresponding to the lines of force of nearby magnetic fields (CORELL, HAZEN and BAHNG, 1956; see also: SEVERNY, 1952; SEVERNY and KHOKHLOVA, 1953). Here the motions are mainly uniform, except those parts of trajectories where the mass is detaching from the main body of prominence or streaming into the «center of attraction», where it is substantially accelerated sometimes up to supergravitational values (BARTLETT, WITTE and ROBERTS, 1953). Finally, the eruptive and tornado prominence show peculiar motions sometimes like a spiral running along the conical surface (resembling the motion of a charge in the field of «isolated» magnetic pole) or along a cylinder (ROTSHILD, PECKER, and ROBERTS, 1955). Some attempts were made to explain these motions as result of diamagnetic repulsion of a big mass of plasma from nearby increasing magnetic field (JENSEN, 1959; see also SEVERNY and KHOKHLOVA, 1953). One peculiarity of prominence motions should be kept in mind. In loops and some eruptive prominences, we often observe a dilatation of the stream as if the ascending and descending motions coexisted together in one stream. This dilatation may sometimes lead to confusion on the real motion of the knot or streamer as a whole with the motion of the front of a knot. We think that this is the cause of the abrupt changes of velocity measured by PETTIT in eruptive and some other prominences.

The spiral-like motions in prominences can be explained if the velocity vector is tilted to the magnetic lines of force, and a current along the direction of motion appears. The mean free path of ions is considerably larger than that of electrons, and if it is compared with the size of the knot, the electrons will be decelerated and the knot will consist mainly of rapid ions and slow electrons that give rise to a current. The motion of this element of current is nearly the same as the motion of individual charged cloud of particles, and so it proceeds along spirals in the field of a magnetic pole (PICKELNER, 1956).

The problem of forces producing the observed motions of prominences remains still obscure; however, there is hardly a doubt about their electromagnetic nature, and supergravitational accelerations and the reverse motions along the same curved path (surges) are the best manifestation of the peculiar character of these forces. Another phenomenon for further explanations is the ability of prominence material to *cumulate* into jets and knots; however, something like a pinch-effect in the presence of azimuthal or toroidal magnetic fields might be used for explanations.

## REFERENCES

- ABETTI, G., 1932, *Publ. R. Oss. Arcetri, Fasc.* 50, 47.
- ARZIMOVICH, L., S. LUKJANOV, I. PODGORNÝ and S. CHUVATIN, 1957, *Žurn. Ėksp. Teor. Fiz.*, **33**, no. 1.
- BABCOCK, H., 1955, *Ap. J.*, **121**, 349.
- BARTLETT, T., B. WITTE and W. ROBERTS, 1953, *Ap. J.*, **117**, 292.
- BIERMANN, L., 1941, *Vierteljahrsschr. Astr. Ges.*, **76**, 194.
- BJERKNES, V., 1926, *Ap. J.*, **64**, 93.
- BUMBA, V., 1959, *Bull. Astr. Inst. Czechoslov.*, **10**, no. 5.
- BUMBA, V., 1960, *Publ. Crim. Astroph. Obs.*, **23**.
- BRAY, R., R. LOUGHHEAD, V. BURGESS and M. MCCABE, 1957, *Austral. J. of Phys.*, **10**, 319.
- CALAMAI, 1934, *Publ. R. Oss. Arcetri*, **52**, 39.
- CHEVALIER, S., 1913, *Ann. Obs. Astron. Zo'se*, **9**.
- CHEVALIER, S., 1950, *Ann. Obs. Astron. Zo'se*, **70**, 234.
- CORELL, M., M. HAZEN and J. BAHNG, 1956, *Ap. J.*, **124**, 597.
- DODSON, H. and R. McMATH, 1952, *Ap. J.*, **115**, 320.
- DUBOV, E., 1954, *Publ. Crim. Astroph. Obs.*, **12**, 46.
- DUBOV, E., 1955, *Publ. Crim. Astroph. Obs.*, **15**, 121.
- ELLISON, M., 1949, *M. N.*, **109**, 3.
- EVERSHED, J., 1909, *M. N.*, **69**, 454.
- EVERSHED, J., 1910, *M. N.*, **70**, 218.
- GOLD, T. and F. HOYLE, 1960, *M. N.*, **120**, 84.
- GOPASUK, S., 1958, *Publ. Crim. Astroph. Obs.*, **19**, 100.
- HALE, G., 1908, *Ap. J.*, **28**, 100, 315.
- JENSEN, E., 1959, *Astroph. Norv.*, **6**, no. 9.
- KOLB, A., 1958, *2nd Int. Conf. on Peac. Uses of Atom. Energy*, rep. no. 345, Geneva.
- McMATH, R., O. MOHLER, A. PIERCE and L. GOLDBERG, 1956, *Ap. J.*, **124**, 1.
- PICKELNER, S., 1956, *Astron. J.*, **33**, 641.
- RICHARDSON, R., 1940, *P.A.S.P.*, **52**, 282.
- ROTSCHILD, R., J. PECKER and W. ROBERTS, 1955, *Ap. J.*, **121**, 224.
- SEVERNY, A., 1952a, *Dokl. Akad. Nauk. USSR*, **82**, no. 1.
- SEVERNY, A., 1952b, *Publ. Crim. Astroph. Obs.*, **9**, 3.
- SEVERNY, A., 1957a, *Publ. Crim. Astroph. Obs.*, **17**, 129.
- SEVERNY, A., 1957b, *Astr. J.*, **34**, 688.
- SEVERNY, A., 1958, *Publ. Crim. Astroph. Obs.*, **20**, 22.
- SEVERNY, A., 1959, *Astron. J. USSR*, **36**, 208.
- SEVERNY, A., 1960a, *Publ. Crim. Astroph. Obs.*, **22**, 12.
- SEVERNY, A., 1960b, *Publ. Crim. Astroph. Obs.*, **24**, 288.
- SEVERNY, A. and V. KHOKHLOVA, 1953, *Publ. Crim. Astroph. Obs.*, **10**, 9.
- SEVERNY, A. and V. SHABANSKY, 1960, *Astron. J.*, **37**, (in press).
- SEVERNY, A. and E. SHAPOSHNIKOVA, 1954, *Publ. Crim. Astroph. Obs.*, **12**, 3.
- SEVERNY, A. and E. SHAPOSHNIKOVA, 1960, *Publ. Crim. Astroph. Obs.*, **25**, (in press).
- STEPANOV, V., 1958, *Publ. Crim. Astroph. Obs.*, **20**, 52.
- STEPANOV, V., 1960, *Publ. Crim. Astroph. Obs.*, **24**, **25**, (in press).
- TYLOR, G., 1950, *Proc. Roy. Soc.*, A **201**, 159.
- ZIRIN, H., 1959, *Ap. J.*, **129**, 414.

## PART IV.

### Considerations on Localized Velocity Fields in Stellar Atmospheres: Prototype — The Solar Atmosphere.

#### C. - Transient Velocity Fields in the Lower Solar Atmosphere.

#### Discussion.

*Chairman:* R. LÜST

— R. LÜST:

We add first a few observations which may be useful for the aerodynamicists; data on the spicules and on the different types of radio bursts. Then we consider the motion in sunspots; it is important to know how sure we are that the motion is along the magnetic lines of force and to discuss the problem if there are motions across the magnetic lines of force. Second, we should discuss the motion in prominences. We are faced with the problem of how important is the magnetic field in the motion of quiescent prominences and in the eruptive prominences. As a third subject we have the flares. Finally, I would emphasize that we should also discuss the spicules, and return to the question whether the spicules are related to the granulation, and are the same phenomena we see in different layers. We see the granules in the photospheric layers, and we see the spicules in chromospheric layers. I think the main problem on flares is their eruption, what kind of forces are involved, and which are the most important features we can explain.

— R. N. THOMAS:

Refer to the accompanying schematic representation of the properties of the spicules, and where they occur on the surface of the sun. For orientation, I have included the height parameter, the temperature parameter, and optical depth parameters—in the continuum at 5000 Å, in the Lyman continuum, and in  $H_{\alpha}$  of hydrogen, so you have an idea where things occur relative to rocket spectra as well as the visual spectral region. First point on spicules—there are a great number of them; they are small objects moving with relatively high speed. Number is something like  $10^4$  at the solar surface at any one time. Speed: ranges between 20 and 100 km per second; 100 is for an abnormal spicule; a spicule reaching the height of 20 000 km is an abnormal spicule. Most of them get to around 10 000 km. The maximum height

reached shows a good correlation with speed. An original impulsive motion decelerated under gravity is as good a representation of time, distance data on spicules as is any. Density in a spicule, we can guess roughly to be  $10^{11}$  protons per  $\text{cm}^3$  down at the height where they first appear around 4000 km. All observations of the spicules refer to heights of 4000 km and above. The theories which have been made on spicules refer to 4000 km and below, where there are not spicule observations presumably because there is so much obscuring material. I don't cover theory here, I simply mention there has been some attempts at constructing several.

TABLE I. — *The spicule system and its environment.*

Height	0	500	1000	1500	4000	20 000
← 500 km to photosphere			← inhomogeneous region begins →		spicule structure appears in broad-band $H_\alpha$ filter	
$T_e$	4000° 4500°	6000°	8000°	$\left\{ \begin{array}{l} \text{cold} - 1 \cdot 10^4 \\ \text{hot} - (2 \div 5) \cdot 10^4 \end{array} \right\}$	proton flux upward in spicule system $\sim 10^{38} \text{ s}^{-1}$	
$n_H$	$10^{15}$	$10^{14}$	$10^{12}$		$10^{11}$	↑ 20 km/s $< V < 100$ km/s
$\tau (\lambda 5000)$	.01	$10^{-5}$	—	—	$n \sim 10^{11}$ ; $T_e \sim (2 \div 5) \cdot 10^4$	
$\tau (LyC)$	—	$10^4$	100	$< 1$	<i>spicule</i> height $\sim 2 \cdot 10^4$ km $< 10^8$ km width spicules cover 1% of surface	
$\tau (Ly\alpha)$	—	—	$10^6$	$10^3?$	↓ $10^4$ total over sun	
$\tau (H_\alpha)$	—	$50 \div 100$	20	—		
$n$ (interspicule) $\sim 10^9 \div 10^8$						
$T_e$ (interspicule) $\sim 10^5 \div 10^6?$						

There are only observations at the limb—nothing on the disk which has been unambiguously identified as a spicule. At 4000 km the typical spicule has an upper limit of 1000 km in diameter. With the density already given, 1% of the solar surface or less is covered by spicules. Yet if you compute the total flux of material in a spicule, using a typical velocity of about 30 km per s, you get roughly  $10^{38}$  protons per s ejected into the solar atmosphere. Warning: one sees spicules going up and coming down again. The figure given is only the number of atoms going up, so that the net number of atoms supplied is something less than that. But there is probably not a factor as large as 10 in the difference, if one takes the number of spicules going out, minus those



coming in. Roughly, there are  $10^{42}$  protons in the entire corona; so roughly in three hours the spicule system feeds in enough material to replenish the corona. The uncertainty in this conclusion is that in the figures given. Character of the medium through which the spicules go, uncertain. It looks very much as if at the height of 10 000 km, the interspicular medium is at  $10^6$  temperature, rather than a small number. I want to emphasize this point relative to the diagram distributed the other day. Basis: observations of the coronal lines at the eclipses, show that Fe X exists at heights of 10 000 km, maybe lower. Maybe already at 4 000 km we have such temperature in between spicules. The situation between 1 000 km and 4 000 km is uncertain. Note that the density of this region is near  $10^9$ , so we have a temperature of  $10^6$  with  $10^9$  protons per  $\text{cm}^3$  between spicules. In a rough way, the problem then is to tie what I summarize here with the structure of the chromosphere and with the structure of the photosphere discussed in the preceding sessions. I will not attempt to talk about the interrelation of structure now, however. Note also, there is a variation in distribution of spicules from pole to the equator, and possibly a variation in orientation; I do not think the question is well-enough settled that I would like to say something about that. I just want to stress, however, the great importance of spicules for the solar astrophysicist in terms of loss of material from the solar surface; the velocities involved; and the interrelation to the medium we have been talking about. From the point of the aerodynamicists, if I look at these velocities and take a thermal velocity corresponding to the table; I see that if these spicules do indeed extend downwards to the lowest chromosphere or photosphere region, I do indeed have a superthermic phenomenon which maybe is similar to a supersonic jet. If the spicules only extend as low as 4 000 km, and between the spicules is just the coronal medium, then the spicules are subsonic phenomena. But this is a point where one has to tie a theory to the structure of the medium. I would like to stress this uncertainty on the medium, and this is the reason I make such a point here of what the character of the interspicular medium is relative to the character of the spicules in terms of the things that one wants to interpret. For more details, refer to the book *Physics of the Solar Chromosphere* by R. G. ATHAY and myself, and the thesis by R. B. DUNN of the Sacramento Peak Observatory.

— R. B. LEIGHTON:

Are spicules not often observed going out at a considerable angle with the vertical and then coming back along the same line?

— R. N. THOMAS:

Yes, the polar spicules are more vertical than the spicules at the solar equator.

— R. LÜST:

Polar spicules sometimes can be seen to have a tilt connected with the tilt of the polar rays.

— K. O. KIEPENHEUER:

If you look at the spicules on the disk, then it appears that the spicules are not distributed homogeneously over the solar surface. On the disk, the spicules can be seen in the  $H_{\alpha}$ -pictures as small dark spots, which seem to be arranged in a kind of network, which obviously is coinciding with the network we observe in the calcium flocculi; the  $\text{Ca}^+$  pictures in the undisturbed region of the sun. The network can last for several days. The spicules seen on the disk seem to have the same lifetime and total number as those seen at the limb.

— R. N. THOMAS:

May I emphasize that the spicules are really only defined on the limb; if I interpret a disk observation and identify it with a spicule, I have already introduced an interpretation of the data. I do not disagree with you that the disk observations may be spicules, I want only to emphasize this point of comparison between observation and interpretation of observations.

— K. O. KIEPENHEUER:

I will survey the different velocities which occur in the disturbed parts of the solar atmosphere.

1) *Flare regions*: We have learned already, that velocities between 0 and 600 km/s have been observed in the bright parts of the flares. From the flare region so-called «surges» or «flare surges» are ejected (appearing dark against the disk and usually brighter than prominences against the sky) with velocities between 50 and 250 km/s. They seem to follow the magnetic lines of force. Most of them return along curved paths to the sun.

2) *Prominences* are cool formations ( $T \sim 5\,000$  to  $10\,000^{\circ}$ ) in the corona ( $T \sim 10^6$  degrees). They show internal motions of 10 km/s or more, have a lifetime of weeks or months, they can rise with velocities up to 700 km/s (thermal velocity in the corona  $\sim 200$  km/s). There is no observational evidence, whether the corona is moving with the prominences. There are different types of effects of flares on prominences (filaments), which work up to distances of several hundred thousand km with velocities of 20 to 100 km/s. An unknown agent is being radiated away from the flare, affecting form, stability and internal motion of the prominence (filament). This effect is obviously

TABLE II. - *Velocities and energies of active sun.*

I) Velocities			
Life	Phenomena	Velocity	Data
Weeks	Plage regions	$\sim (0.5 \div 2)$ km/s	Doppler
Days	Sunspots	$(0.5 \div 8)$ km/s	(Doppler, away from center of disk)
Months	Quiescent prominences	Internal motions 10 km/s	(Doppler and visible displacement)
Hours	Ascending prominences	$(50 \div 700)$ km/s	Displacement
Hours	Coronal motion (internal)	10 km/s	
Minutes	Coronal whip	$\sim 600$ km/s	—
Minutes	Flares	Internal motions $(0 \div 600)$ km/s	Doppler
Minutes	Flare surges	$(50 \div 250)$ km/s	(Doppler and visible displacement)
Minutes	Steady streams and flows of gas producing sequences of terrestrial disturbances	$\leq 1000$ km/s	
Minutes	Effect of flares on existing prominences	$(100 \div 1000)$ km/s	
Minutes	Effect of flares on triggering other flares	$(1000 \div 1500)$ km/s	—
Minutes	Radio bursts type II (flare associated)	$\sim 1000$ km/s	—
Seconds	Radio bursts type III	$\sim \frac{1}{3}c \div \frac{2}{3}c$	—
Hours	Radio bursts type IV (flare associated)	Highly correlated with ensuing geomagnetic storms, $V \sim 500$ km/s	
Hours	High speed gas generating magnetic storms $\leq 1500$ km/s		

II) *Energies*

Large flare:  $> 10^{32}$  erg (radiated energy)  
 $> 10^{30}$  erg (particle emission, 10 MeV  $\div$  30 GeV per proton)

Radio emission from large flare  $\sim 10^{27}$  erg  
 Ejected mass  $\sim 10^{19}$  g (total  $\sim 10^{34}$  erg)  
 Total energy content of quiet corona  $> 10^{32}$  erg implies  
 annihilation at 500 G in the flare

being propagated through coronal volumes with densities of  $10^8$  to  $10^9$  protons and electrons/cm<sup>3</sup>.

There is another effect of importance:

3) When a flare is occurring, the probability that other flares will occur in the neighborhood is larger than random. This can only be explained by saying that one flare is triggering another flare. And this triggering effect has been observed all over the hemisphere, mostly by BECKER. The velocity of this triggering effect, which is propagating along the surface of the sun, probably in the corona, is about 1000 km/s. It could be occurring in a lower layer but then it is more difficult to understand this velocity. It might be a « solar quake ».

4) Then we observe after the flare, on the earth, a geomagnetic storm, and this storm obviously is produced by clouds of corpuscles which are ejected somehow from the flare. The travelling velocity of this cloud of corpuscles, deduced from the fact that the geomagnetic storm starts about one day after the flare, turns out to be of the order of 1000 to 2000 km/s.

5) Now I come to the radio bursts, which give us the possibility of deducing some velocities. Let me, in a few words, explain radio burst: It is assumed that something is travelling through the corona upwards from the flare, or in any direction from the flare, exciting plasma oscillations in the corona, the frequency of which depends on the electron density in the corresponding path. So some agency moves through coronal regions of decreasing density if upward, increasing if they go down, and therefore the frequency of the emission will change correspondingly. If we observe the change of frequency as a function of time, we will be able to deduce the velocity in the corona. From this simple idea the type II and the type III radio burst can be understood. For the type II radio bursts, obviously something must move away from the flare again with a velocity of the order of 1000 km/s. This velocity, at least for the type II burst, has been confirmed by interferometric observation; so they could follow the transmitter through the corona and they were able to tell something about the orbit. This phenomenon takes a few minutes.

6) There are shorter living phenomena called type III bursts which correspond to velocities of  $\frac{1}{3}$  or  $\frac{2}{3}$  of the velocity of light. These phenomena which are much faster last only a few seconds. The type II and type III occur after or specifically during flares. We must imagine that something is moving through the corona with this speed; I have to add that type III bursts occur not only with decreasing frequency, *i.e.* with increasing height, but also are observed coming down. So some of the bursts are started in the high corona and then move downward; and in other cases this burst has the shape of a U, called



U-bursts, which means that the travelling agent is coming up and then coming down again. This may be interpreted saying that something is moving along the lines of force.

7) Then there is another motion in the corona which has been observed at Sacramento Peak with the coronagraph; there was a kind of « whip » motion of a streamer changing in a few minutes from one static configuration to another. The velocity which is necessary to explain this deformation turned out to be of the order of 600 km/s. This is the only case, to my knowledge, that a motion of such a high velocity of the coronal matter in the corona has been observed optically. There is no evidence if this is a motion of matter or a motion of an excitation wave or something like that.

— A. B. SEVERNY:

I would like to add other phenomena connected with the flares. The first one is the outburst of cosmic rays with energy of about 10 GeV; and the second one is the outburst of more slow protons, which was measured during the IGY, protons with energy of about  $10^6$  eV. One more phenomenon is the measurements made by LYOT, ROBERTS, WALDMEIER *et al.* connected with the motions in the green corona. This is specific coronal emission, and the motion of the knots which may be observed in the green line give a velocity which is not very high, something around not more than  $(10 \div 30)$  km/s. This may be interesting because some people tended to identify the motions in the corona with the corpuscular stream itself.

— R. B. LEIGHTON:

In studying the effects of flares at some distances away from where they occur, there is one effect which appears in some motion pictures taken at the Lockheed Observatory (\*) and which seems quite striking.

There is a flare at some point on the solar disk, and some distance from it there is what is called a disk filament, which is dark and narrow region (as seen in  $H_\alpha$ ), which remains quiet for many days, perhaps for several solar rotations. Then it often occurs that shortly after the eruption of the flare, a certain part of this filament will be « evaporated » and suddenly disappear from the disk filament. Many examples of this have been observed in the Lockheed Observatory film and characteristic propagation speed is about 1000 km/s. So I add an eighth point here: flare effect on filament—about 1000 to 1500 km/s. I may mention also that for the same flare where the filament disappeared, one sometimes can also see one or more other flares at some distances away which are sometimes called « sympathetic » flares; as

(\*) R. G. ATHAY and G. E. MORETON: *Ap. J.*, in press.

KIEPENHEUER said, these are statistically unlikely to occur independently and certainly they suggest that somehow, whatever instability made one flare also made the other one, possibly by a propagation of the «trigger» in each direction from some point which could be remote from both flares.

— K. O. KIEPENHEUER:

I want to add another point, namely, that the effect on filaments from flares is not isotropic, it can happen that one filament which is close by the flare is not affected at all, and another in another direction which is far away, is strongly affected. From that one may possibly infer something about the nature of the force acting.

— R. B. LEIGHTON:

I am sure it is connected with the problem of propagation along magnetic field lines.

— H. PETSCHKE:

Why associate radio frequency bursts with plasma frequency instead of with cyclotron electron frequency?

— F. KAHN:

Radiation can be propagated in an ionized gas only if its frequency  $\nu_{\text{rad}}$  exceeds the plasma frequency  $\nu_{\text{pl}}$ . For radiation of a given frequency, the opacity of the medium increases with  $\nu_{\text{pl}}$ . The maximum contribution to the energy in a given ray therefore comes from the level where  $\nu_{\text{pl}}$  is largest. For a ray leaving the solar corona radially this occurs where  $\nu_{\text{pl}}$  and  $\nu_{\text{rad}}$  are equal.

— F. H. CLAUSER:

If we have a non-linear wave propagation outwards, and if it decreases in intensity as it moves outward, then you should observe a change in speed. Do you observe such change in these observations of triggering action?

— K. O. KIEPENHEUER:

I think it is impossible to get such information from the few data existing, but from the radio bursts it turns out that the velocity does not change in spite of the fact that the density change is a factor  $10^3$  along the path of the burst. Such information is really important for fixing the nature of these phenomena.

— M. MINNAERT:

There is a narrow relation between items 4 and 5; so that we see that the geomagnetic phenomena are closely related to type II bursts.

— K. O. KIEPENHEUER:

This is very probable; however, there are more features than type II bursts. It could be indeed that even 3, 4 and 5 [3 (triggering of flares), 4 (travelling speed of corpuscles from sun to earth), 5 (type II burst velocity)] might be the same because of the same velocity of about 1 000 km/s. We do not know for certain yet.

— W. H. MCCREA:

Are there other regions except flares from which particles are coming?

— K. O. KIEPENHEUER:

To my knowledge, there is nothing else besides a flare which does such a thing; because all of the bursts are correlated with flares.

— M. KROOK:

Type III bursts are very important in solar activity, and they very often occur when there is no visible activity on the sun; so one can assume that there are small, very very small flares which are not visible in  $H_{\alpha}$ , but which will trigger type III bursts.

— R. LÜST:

Could you comment about polarization of the bursts?

— M. KROOK:

The information is complicated and inconclusive. Neither Wild nor Maxwell is very keen on committing himself at the moment about polarization information. It is a very difficult measure to make.

— C. W. PECKER:

Are not type IV bursts important in this matter?

— K. O. KIEPENHEUER:

Type IV bursts are supposed to be clouds of matter floating in the corona; going up with the velocity of several hundred km/s; radiating because of the synchrotron mechanism; so it is assumed that there are electrons of some million volts in these clouds and there must be a magnetic field of the order of a few gauss in order to explain the intensity of the radiation and the observed strong polarization.

— R. LÜST:

This type IV bursts are really very strongly polarized, nearly 100%.

— M. KROOK:

They are a continuum as compared with the comparatively narrow types II and III.

— C. DE JAGER:

It is good to remark that it is not the type II bursts which are principally correlated with geomagnetic storms, but rather the type IV bursts.

— F. H. CLAUSER:

You observe things moving along a magnetic field under gravity; if you try and deduce a magnetic field of the whole sun, do you get consistency from day to day; or do the magnetic patterns change so much from day to day that there is no pattern?

— K. O. KIEPENHEUER:

In the outer part of the corona, there are very few observations so that we have not yet direct knowledge of the variation from day to day. In the inner part, around the sunspots, there are evident variations from day to day in the shape of prominence motions, in the shape of the ejection of surges, and also from the change of polarization from day to day in certain radio observations.

— H. LIEPMANN:

In the same line as Clauser's question, can one say anything about inconsistency between the steady magnetic field of the sun, and a field produced by fluid motion? *I.e.* are the observed field lines and the observed streamlines free of contradiction?

— R. LÜST:

If somebody has a good answer to this question, we would already be near the solution to a number of these problems.

— J. C. PECKER:

I have two feelings when looking at prominence motions. Very often you have a prominence flowing in a certain way, apparently following magnetic lines of force, or approximately so, and the prominence disappears after some time; quite often it reappears later, showing the same pattern of motion. The second point is that a very common thing is that knots in prominence motion are spiralling, possibly around the magnetic lines of force. Evidence for following the lines of force does not seem to me conclusive.



— L. BIERMANN:

With regard to the spiralling motions, or what appears as a spiral motion in the prominence, which sometimes occur, we have directed our attention in Göttingen and in Munich to the resemblance of the observed pattern to the force-free fields which have been investigated by LUNDQVIST, LÜST and SCHLÜTER, and others. We would like to suggest the possibility that the observed motion in such prominences, which appears as helical, implies that the motion is guided by magnetic fields of the force-free variety. I think this observation is one of the indications that such fields really do occur in nature.

A second point is Liepmann's question as to whether or not in general one could speak of consistency between what one could derive from the motion themselves and from the different information about the magnetic field from spicules, spots, etc. As far as I know there is consistency in the sense that we are unaware of any violent discrepancy between the information which could be derived from the several sources, which are available. But we have to keep in mind that the observations regarding the magnetic field are of such a kind, that we get no unique information about geometrical properties. We can only put together different pieces of evidence, make a reasonable guess, and compare again with what we see. As far as I know, no obvious inconsistencies appear to exist.

— F. H. CLAUSER:

Does anything like a dipole structure appear in the steady state?

— L. BIERMANN:

For instance, we know the following: the field of a sunspot is what you would expect if you would regard the spot as a source or sink of magnetic lines of force and arrange the currents in some way along and below the surface. I think what you would see in the prominences is reasonably consistent, with the picture that one gets from the photospheric observations. Ordinarily it looks as if there would be no strong currents above the surface affecting the magnetic field. Exceptions, of course, are the indications for fields of the force-free variety; the force-free fields are not potential fields because of the currents which flow along the lines of force.

With regard to the dipole character of the « general » magnetic field of the sun, things have become very complicated since the recent discovery by BABCOCK of two years ago, which revealed that the pole fields of the whole sun have reversed their polarity. This makes somehow doubtful the earlier suggestion that this polar field has to be regarded as dipole field of the whole sun. Now there are some suggestions as to the answer, but this should perhaps be excluded from the present discussion.

— E. SPIEGEL:

A question to SEVERNY—about the azimuthal field you described in sunspots. I wonder if you would elaborate your argument for their existence, and describe their distribution if you know anything about that, and suggest the magnitude of the azimuthal component in sunspots?

— A. B. SEVERNY:

I am sorry, I wish I could do that but we do not have any method to measure the azimuthal field.

— E. SPIEGEL:

You suspected the existence of a non-radial component?

— A. B. SEVERNY:

It is just a guess. I find some indication of possible existence of this azimuthal field, and this indication is the following: We tried to establish this azimuthal field by scanning the spots near the very border of the sun. If we have an azimuthal field, then we must have a radial, for instance, a north component at this border of the spot in an upward direction, and from the scan through the south border we should get a downward direction of the field. Thus, by observing the spot at the borders of the sun, we can try to establish this component. I have some indications of the existence of this field, but they are not conclusive as yet. This is what I mean; but I am sorry we don't yet have methods permitting us to determine the azimuthal component of the magnetic field; this is a very hard job. Now, I would like to add a few words regarding Clauser's remark.

There were observations made at Boulder which show that the assumption of dipole field for spots is in satisfactory agreement with the observed motion of prominences. As far as I can remember the picture, it was considered theoretically that the dipole field is a little below the surface; and they calculated the field lines picture at different inclination of the dipole to the solar surface. The observed motions were compared with the location of dipole field lines and they found pretty good agreement between the two.

— J.-C. PECKER:

J.-L. LEROY in Meudon made very nice measurements of the transverse field in sunspots using polarimetric measurements. This, of course, is very important because, between the time it is at the center of the disk and the time it is at the limb, the spot and the magnetic field of the spot can be changed. If you have a method to measure transverse magnetic field, then you can measure topography of the field at the same time. Actually, many spots have

been measured this year, and I think the fine resolution of the magnetic field is quite good. I don't have here many details.

— A. B. SEVERNY:

The problem is not to measure the inclination of lines of force, because we can measure inclination of magnetic force just using the simple Seares' formula. This was applied at Mount Wilson Observatory since 1920; your method is, of course, better; but how to determine the azimuth of the projection of transversal field, this is the question, that has not been solved as yet; that's what I mean.

— J. TUOMINEN:

The basic problem of the appearance of sunspots is more a problem of the solar interior than one of the solar atmosphere. Now I should like to ask especially SEVERNY: Do the velocity and magnetic fields in sunspots give any indication on 1) whether a long-lived sunspot is a phenomenon which is continuously coming out from its source beneath the photosphere? or 2) is the sunspot a phenomenon which once comes out from its source beneath the photosphere and then lives a shorter or longer time at the solar surface? This question is important, for instance, when movements of sunspots are studied.

— A. B. SEVERNY:

Except for the Evershed effect I mentioned in my talk, and disregarding fine structure, we observed sometimes a lifting of the whole region connected with the spot; but, owing to the bad seeing, we don't have indications on fine structure of this motion. And, of course, in most of the cases these motions are also masked by the usual Evershed pattern, and we cannot distinguish clearly the motion of the spot as a whole and the Evershed pattern.

— R. LÜST:

As far as I remember, the measurements from the Evershed effect of the motions in sunspots had given velocities of the order of one to two km/s. Now SEVERNY has reported velocities up to about 8 km/s. Could he comment?

— A. B. SEVERNY:

These velocities are higher than the velocities reported in the usual literature, because they refer to a fine structure, and are determined by careful study of the line-profile.

— R. B. LEIGHTON:

Is it not true that the 7 or 8 km/s refers to a very small region at the outer boundary of the penumbra and not to a velocity distributed over the disk

of the sunspot as found by EVERSHED? Second, does the velocity correspond to outward flow or inward flow of matter?

— A. B. SEVERNY:

These high velocities correspond to the outer boundary of the penumbra. The sign of the velocity follows precisely that found by EVERSHED. The  $U$  is the umbra in the spectrum and  $P$  is the penumbra (see diagram). BUMBA found that if we try to draw the form of spectral lines, something like the following picture called by him the flag phenomenon takes place. He has not been able to measure the motions in the umbra itself and his results refer only to the penumbra. This is what I am speaking about.

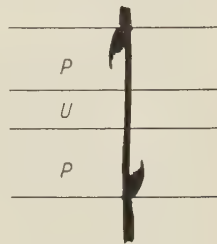


Fig. 1.

— J. RÖSCH:

Concerning the Evershed effect—you may have noticed that on the film by SPIEGEL several days ago there were motions just at the edge of the boundary between the penumbra and the umbra. We have also found in our first attempt to have moving pictures of the granules and sunspots, something of this sort. I think this is a point which must be looked into very carefully, because my feeling up to now is that one will find here, just on this edge, velocities higher than the average velocities inside the granules and inside the penumbra. I should not be surprised that here on this edge we find velocities of several km/s and this will be quite consistent with the velocities indicated by SEVERNY.

— J.-C. PECKER:

Either there is a correlation between thermodynamic structure of the spot in penumbra and umbra, and the motion, or there is not. Now from the recent measurements made in Meudon, I think that the pattern of Evershed motions measured by SERVAJEAN show little correlation, for complex spots, whatsoever with the shape of umbra or with the penumbra also. There is a very strange pattern which looks more like the maps of the magnetic fields that SEVERNY showed which also show no strong correlations with shape of the umbra or the shape of the penumbra.

— R. B. LEIGHTON:

We have also studied motions around sunspots with our Doppler device. If we observe a spot with both umbra and penumbra near the center of the disk, it is invariably true that the velocities from  $H_{\alpha}$ , the Na  $D$ -lines, etc., are essentially zero within the outer boundary of the penumbra. We find no evi-



dence for motions on a scale larger than  $1'' \div 2''$ , which would exceed 0.1 km/s. However, just *outside* the penumbra one finds very pronounced radial lines of flow, starting at the boundary of the penumbra and shading off as we go away. If the sunspot is seen foreshortened near the limb, one sees the same thing. However, in the  $\lambda 6103$  line of calcium, there may be indications of an outflow or inflow of matter—generally the former from our observations. In  $H_{\alpha}$  there will be opposite directions of motion on the two sides of the spot; we interpret this to mean the gas moving in toward the sunspot is moving very nearly *horizontally*, along the surface of the sun. Schematically, I visualize a picture of sunspot structure consistent with this, as follows. We have the granulation outside the spot, the penumbral region following the converging lines of force toward the umbra, the penumbra making a small angle with the solar surface. The hydrogen, being free to move along lines of force, moves essentially in a vacuum outside the penumbra, but is suddenly stopped when it strikes the denser atmosphere at the boundary of the penumbra.

Note that we sometimes find an eruptive prominence coming from some point on the edge of the penumbra. MICHELSON observed these first and you will find accounts of them in the early issues of the *Astrophysical Journal*. He pointed out (from his visual observations) that these start from the outer boundary of the penumbra. But we also find that in an eruptive prominence not only is there an outward motion, but also often a component of *rotation* so that one edge of the prominence may be moving toward us and the other side away, as if there were spiralling around lines of force. This has been seen several times, although it is by no means a universal property of eruptive prominences.

Continuing to another point, we have obtained some interesting results on the  $K$  line of  $\text{Ca}^+$  which, although the interpretation is not completely clear, seem sufficiently striking to be pointed out here. We took a spectroheliogram of the solar surface; however, as the slit scanned the solar surface, we also scanned the slit past the spectral line, so that we obtain a combination spectrogram and spectroheliogram. It has been known for a long time that the  $K_2$  emission is stronger in the *violet* component of the  $K_2$  line. I think we have tracked this down to a difference in the *kinds of features* which produce the emission. We see many sharp, bright points of emission scattered about the disk, and these are more numerous and brighter in the violet component of  $K_2$  than in the red. The slide shows the effect. One-half was centered on the red component of the spectral line and the other one on the violet; they are otherwise (with respect to the exposure and all the subsequent photographic treatment) identical, except in being different regions on the sun. You see that there are many more regions per unit area (many of them very tiny) which emit light on the violet side of the line than are present on the red side of the line.

I suggest that the calcium emission comes from regions which may be very small, in many cases rather point-like, and which are preferentially moving upwards as they emit. I think these might have something to do with spicules.

— J.-C. PECKER:

I want to draw attention to one point, which must be considered when discussing the dynamics of a sunspot; *viz.*, empirical determination of the gas pressure at a given geometrical level. Because there is a magnetic field, the gas pressure within and outside the spot may of course differ. The point is, that present measures of this differential gas pressure do not seem conclusive. The measurements by MICHARD of some years ago gave a pressure in the umbra which was equal to about 0.2 that in the photosphere; a rough estimate of magnetic plus gas pressures gave them equal to the gas pressure in the photosphere. From more recent measures by LABORDE, it seems that this was wrong, and that the pressure in the umbra is actually much bigger than it was thought before, at the same level, and is almost of the same size as the pressure in photosphere. These types of data are important to the problems being discussed today; I want to emphasize how difficult it is to obtain results.

(*Ed. note:* There followed an inconclusive discussion between ELSTE, PECKER, MINNAERT on reliability of relative geometrical scales within and outside sunspot.)

— E. SPIEGEL:

I'd just like to ask how LEIGHTON envisages the general mass flow around a sunspot; in particular, how does he satisfy the conservation of mass, what are the sources for the inflow, where does the mass go, and such questions.

— R. B. LEIGHTON:

When I discussed our Doppler effect measurements I showed a plate of the motions far from the center of  $H_{\alpha}$ , all over the disk, and these were predominantly downward motions. I think that when one looks at limb prominences with Lyot filters, one sees predominantly downward motions in the quiescent prominences. I would regard the inward flow, seen in  $H_{\alpha}$ , to the outer boundary of the penumbra as being merely another example of the general downward flow of the hydrogen gas from the corona. Now the reason motion stops at the boundary of the penumbra, it seems to me, is that the density increases so greatly that to keep the conservation of mass the velocity correspondingly decreases and goes below our resolving power.

— E. SPIEGEL:

Would you think then that the penumbra is basically cool with bright streaks resulting from the inflow of hot material?

— R. B. LEIGHTON:

No, I don't think the bright streaks have anything to do with the part of the material that is coming in. I think these are probably convective cells that are seen edgewise. The material has to move along the lines of force; I'm assuming that the lines of force go in that direction, which seems likely. So I think of the bright lines being convective cells and, as the lines of force leave the surface, you just have a boundary region to which the hydrogen can come more or less freely from the relative vacuum in the chromosphere and corona outside. This is a qualitative picture, and there may be some quantitative difficulties with it which I haven't yet discovered.

— E. SPIEGEL:

If you're thinking of convection, I can't understand why it circulates in that way. The convection in the model for sunspots that I've mentioned previously by DANIELSON is supposed to be occurring in rolls, the rolls having their axes radially in the penumbra, and these correspond to the filaments in the film that was shown. And here now, we're confronted with another kind of flow, transverse to that presumed convective flow. This has also been called convection and it is, I think, confusing.

— A. UNSÖLD:

Perhaps just a word of explanation. Looking at the motions in a sunspot from a hydrodynamical viewpoint, one should be aware that the outward motion which one sees in the Evershed effect of the usual metallic lines is really the main phenomenon and comprises by far the largest mass. What one sees as inward motion in  $H_{\alpha}$  and in  $H$  and  $K$  takes place at about the level of the spicules; it is only a secondary phenomenon. Observations also show that the whirls which one sees on the  $H_{\alpha}$ -spectroheliograms have nothing to do with the magnetic field but are simply determined in the same way as the circulation in terrestrial cyclones and anti-cyclones by the Coriolis force.

In a cross-section things would look approximately like Fig. 2. In the main level of the photosphere, the motions must have come up near the umbra and then go outwards. And only in the high level the spicules—or one may say just as well very small prominences—move inwards. You should compare my remark to the well-known observations that also large prominences are frequently drawn into the sunspots. The sunspots exerts—we don't quite understand how—an attractive force on prominences, and what we see as inward motion in  $H_{\alpha}$  and  $H$  and  $K$  is evidently a phenomenon of the same nature. This is something quite different from the outward motion in the ordinary Evershed effect, which gives the  $(6 \div 8)$  km/s near the outer edge of the penumbra and velocities of about 2 km/s nearer the umbra. If we try

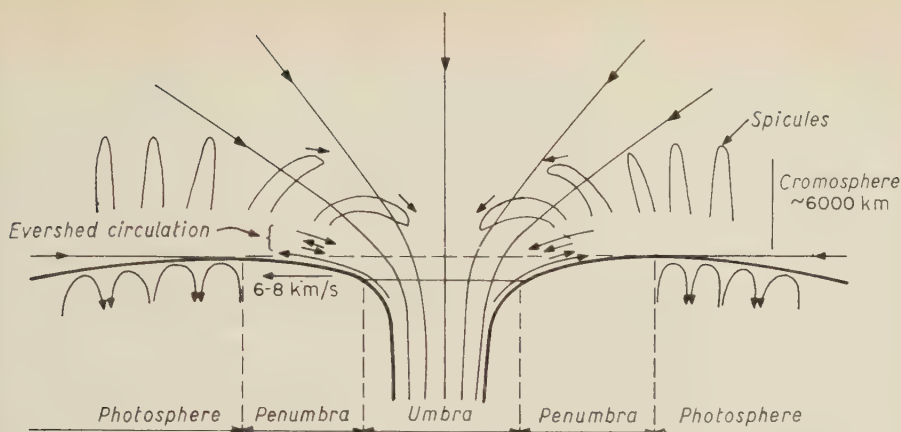


Fig. 2.

to collect our ideas into a hydrodynamical theory of sunspots, we should in any case begin with these outward motions and try to complete this circulation somehow. The inward motions are evidently a secondary phenomenon. But this is a wholly theoretical matter.

— A. J. DEUTSCH:

Do you ascribe the vortical patterns that we see in the  $H_{\alpha}$ -spectroheliograms to Coriolis force acting on those outward motions?

— A. UNSÖLD:

The spicules are pulled in by the same force which pulls in the large prominences, and that motion is accompanied by a Coriolis force giving the right curvature. There is no connection with the sign of the magnetic field.

— R. B. LEIGHTON:

It seems to me that what is primary and what is secondary in importance depends upon which part of the sun you're interested in. For many purpose we're interested in what happens outside, where the prominences are, and it seems to me that what the hydrogen clouds are doing out there makes a difference. Also, we are very much interested in establishing, as far as possible, the relations—if any exist—between these motions in outer and lower regions. So first, let us be a bit more definite on the motions in the lower regions.

(*Ed. note:* There now followed a disordered discussion trying to establish which lines showed outflow, which inflow, and the place of origin of these lines. Unsöld's diagram has been expanded a bit following Leighton's suggestions and the «consensus» from the floor. Mrs. BÖHM-VITENSE estimated mean



depths  $\tau_c \sim 0.1$  for the lines showing the Evershed pattern; no estimate was placed on differential depth between lines showing inflow and outflow in the Evershed pattern. Again, the question of differential height within and outside a sunspot of the same line was raised and not answered.)

— R. B. LEIGHTON:

With this information on hand, I would now remark that the motions in  $H_\alpha$  that we observe occur at a much higher level and presumably correspond to motion downward along lines of force that aren't just the ones that go out of the photosphere, but the neighboring ones as well. It is, however, significant, I think, that the boundary of the motion for the inward-moving  $H_\alpha$  is geometrically the same, as far as we can tell, as the boundary of the sunspot as seen in integrated light, which perhaps implies only a small height difference in the atmosphere for these two things—much less than indicated in the figure. Prominences may come in along the lines of force as well.

— A. B. SEVERNY:

In this connection, I would mention that together with BUMBA we measured magnetic fields in the chromosphere above the spot. We reported in *Observatory* two years ago that from measures of the Zeeman effect in the center of  $H_\alpha$ ,  $H_\beta$  and  $\text{Ca}^+ H$  and  $K$  we found an upper limit of some 500 gauss (as compared with some 3000 gauss in the spot—*ed.*) My results this morning showed records of the field in the chromosphere above the spot of some 60 gauss. So if we compare the kinetic energy in the chromosphere with the magnetic energy in the field above the spot, the former is a little smaller. So above the spot, we have a picture in which the magnetic field organizes the motion in some way.

— C. DE JAGER:

In drawing the magnetic field lines, they are assumed parallel throughout the star body and then they diverge suddenly close to the surface. Drawings like this are often found both in scientific and in popular accounts. It should be made clear, however, why we do it like that. Intuitively, one would think that the lines diverge at the limb because the sun «ends» there. But it is clear, of course, that the limb of the sun is only the point where the solar body changes from opaque to transparent; this has nothing to do with the magnetic field, and the density decreases continuously outward there as smoothly as it does 1000 km higher or lower. From that point of view there would not be the slightest reason to assume the field lines parallel just to the sun's surface and to let them diverge higher up. If there is a reason to make them divergent in the chromosphere they must be divergent too in the lower regions.

The only reason I see for making them divergent just at the limb is because there the solar matter is mainly neutral.

— R. B. LEIGHTON:

De Jager's point is well-taken, but it seems to me that the visible boundary of the sun represents more than just the place where the light comes from. It also represents the place where the density is increasing so rapidly downward that it and the pressure go up to enormous values only a few hundred km below the visible surface, to values which can very well provide the pressures which it takes to constrain lines of force of the order of 3 000 gauss strength. So, while it is quite true that we don't know within several hundred km just the height at which this can take place, several hundred km in height is very small compared with the many thousands of km size of a sunspot.

— L. BIERMANN:

I would like to make three points connected with the discussion thus far. First, relative to the Evershed effect, it seems to me that after the report of SEVERNY, there is no reason to believe any more that there is a sort of average motion across the magnetic lines of force. Such motion would contradict the constancy of the magnetic flux, which is to be expected from the value of the electric conductivity.

Second point was the inhibition of the convective energy flux by the magnetic field: that came up already in the discussion. I think the current picture of why a sunspot appears dark is that outside the spot, underneath the photosphere, the energy is carried largely by convection whereas in the spot the convection is affected strongly by the magnetic field, which is strong compared to the kinetic energy of turbulence. It is gratifying to see from the discussions which we had in the last few days that while the whole theory of convective energy transport has become considerably more complicated than astrophysicists usually believed, it is obviously now well within the range of theoretical possibilities to have motions—certain types of convection—of several km/s in the spots as well as outside the spot, but—in the presence of the strong magnetic fields—no energy transport in the spots, but effective transport outside the spots. This is, of course, no answer; but simply an emphasis on a problem which is essentially a theoretical problem, which is very important in the theory of sunspots.

The third point has, as far as I recall, not come up in the discussions. It is the following: As was discussed already a long time ago by COWLING, the appearance of a spot on the solar surface must mean that the magnetic field is carried to the surface by mass motion. I don't know whether any observations which were discussed, or which are possible now, give any indication of mass motions connected with the appearance of a spot on the surface or

the disintegration of a spot to a magnetic patch, or the disappearance of both to the normal quiet state of the photosphere. One would not expect very large mass motions; just as a guess I would expect something of the order of 0.1 km/s or so, so it might well be below the level of observation. But in addition to all the observations which have been and are being carried out, I would suggest that particular attention be given to events of this kind, that is to say to phenomena which are appearing during the birth of a sunspot and during the later stages in which it changes its large-scale structure.

— H. LIEPMANN:

Why do you say the convection is inhibited? I can see that turbulence is inhibited, but if you have a large mass motion from the center on up, should that motion be inhibited as well?

— L. BIERMANN:

I think the convective motion that shows up an Evershed effect is probably connected to a very thin layer, and so it is not at all obvious that the mass is really considerable, and that the energy which is connected with it plays any particular role. The idea I discuss is connected with the state of observations of about 10 years ago. At that time the observers told the theoreticians that in a spot there was no turbulence. And on that basis it was suggested that the absence of turbulence was brought about by the magnetic field, and therefore no energy transport by convection. Now, recently, it has become apparent that in the umbra there are both structures and motions, and therefore we have the somewhat more complicated problem that we have a type of motion which probably differs inside and outside the spot. The theoretical problem which I emphasized was to get more insight into the conditions under which convection—in the presence or otherwise of a magnetic field—can or cannot carry energy.

— F. M. CLAUSER:

If you carry this 8 km/s motion back along a magnetic line this indicates that material is being brought up from below. This is convection.

— L. BIERMANN:

Well, this may be suggested tentatively, but I think everything we know is consistent with the possibility that actually this outflow is a phenomenon in quite a thin layer as compared with the dimensions, and therefore the velocity which we have inside the spot—from the divergence, from continuity, is very much smaller than you would infer from what you observe here on the edges.

— F. H. CLAUSER:

Yes. It may be small, but it is still a velocity from in to out and doesn't this carry hot material out?

— L. BIERMANN:

Yes, but the density is so small that for this purpose it can be neglected. But I must confess I have not made this estimate. Anyhow, it is not available.

— E. N. PARKER:

One can begin with Biermann's point, that the strong magnetic field in a sunspot inhibits the convection so that the convective transport beneath the spot is at a somewhat lower rate than in the normal convection zone. Since this gives a lower temperature in the interior of the sunspot, it is easily shown that the field is further increased, and convection inhibited even more, etc.

Consider a spot which is perhaps 10 000 km across and presumably therefore of comparable depth. Now, 10 000 km at the temperatures you see on the sun is something like 30 or 40 scale heights, the scale height being the vertical distance over which the pressure drops by a factor of  $e$ . Suppose that I have a very weak column of magnetic flux within which it is slightly cooler than outside because of convection inhibition by the magnetic field. Thus, the scale height inside will be less than the scale height outside. Now the pressure inside drops off rapidly, starting down at some base level deep in the sun. And so 30 scale heights or 50 scale heights above the base level, the gas pressure inside the field is considerably lower than the gas pressure outside, even though the temperature difference may be very slight over the entire range. Suppose the temperature is only 1 or 2% different between the inside and outside. In 50 scale heights, you still get a factor of 2 between the pressures. We must have, of course, hydrostatic equilibrium. That is, pressure outside (if you neglect curvature of the lines of force) must be equal to pressure inside plus  $B^2/8\pi$ . Now if you have too low a pressure inside, the pressure outside merely caves in the tube of flux squeezing the material out along the lines of force and increasing the field intensity  $B$ . The outward flow of gas goes on until the increasing  $B^2/8\pi$  makes up for the deficit. Increasing  $B$  further inhibits convection, cools the gas inside the field, so that the pressure drops still more, etc. In this way the spot develops. This is the extension of the arguments which BIERMANN began with his remark that the strong field inhibited the convection.

— E. BÖHM-VITENSE:

I would just like to make the point that this is, of course, only right as long as the temperature in the spot is lower than the surrounding photosphere.



But if there is really no convective energy transport, or at least much less than in the surroundings, you can calculate very easily that already in relatively high layers one gets a temperature which is higher in the spot than in the surroundings.

— E. N. PARKER:

If convection was stopped completely you are right, but presumably it is only partially inhibited.

— E. BÖHM-VITENSE:

In any case, I think this is what you would expect, because if the convective energy transport is stopped somewhere below, then you would expect the temperature to rise below this level, because the flux gets stuck there and will heat the layer below the sunspot. Nevertheless, I would think that the Evershed effect might be a non-stationary phenomenon because I think the sunspot cannot ever reach a stationary state. One can calculate the time which is needed for a disturbance in temperature to reach the higher layers of the spot. If the spot would be a few thousand km deep it would be nearly 1000 years. And since the spot only lasts but a few weeks, I think the spot cannot be in a stationary state. So I think the Evershed effect may very well just show that the spot is not in a stationary state. If the gas in the deep layers of the spot gets heated, then, of course, you have to push up material in order to keep the equilibrium of pressure in those deep layers.

— V. D. SHAFRANOV:

It seems to me, that there are some arguments in the theory of hydromagnetic equilibria, which support the idea of an azimuthal magnetic field in sunspots, developed in the report by A. SEVERNY.

Let us assume, in accordance with the Alfvén idea, that the magnetic field of a spot is a ringformed toroidal flux-tube, emerging out of deep layers. As is known, the magnetic force lines tend to contract, so a force of attraction  $F_1$  arises which is  $F_1 = (B_{\parallel}^2/8\pi) \cdot \pi \alpha^2/R$  per unit length. In this case the radial velocity of the ring must be of the

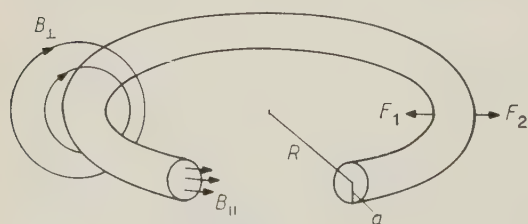


Fig. 3.

order of

$$r_r \sim \frac{B_{\parallel}}{\sqrt{4\pi\rho}} \cdot \sqrt{\frac{\alpha}{R}} \gtrsim 1 \text{ km/s},$$

i.e. much higher than the observed velocity of approach of spots. So

it is necessary to have for equilibrium a current along the ring, which produces an azimuthal field  $B_{\perp}$ , and the force  $F_2$ , opposite to the force  $F_1$ .

I have proposed (*Žurh. Ėksp. Teor. Fiz.*, **33**, 710 (1957)) that there may exist an equilibrium stable configuration in a fluid, having just such structure. It is reasonable to suppose, that a spot represents a cross-section of this configuration.

The connection between azimuthal and longitudinal fields in this configuration is as follows

$$B_{\perp} = \frac{B_{\parallel}}{\sqrt{\ln(8R/\alpha) - \frac{1}{2}}}, \quad \text{for } B_{\parallel}^2 \approx \text{const over cross-section},$$

$$B_{\perp} = \frac{B_{\parallel}^{\max}}{\sqrt{2[\ln(8R^3\alpha) - \frac{1}{4}]}}, \quad \text{for parabolic distribution of } B_{\parallel}^2.$$

The difference between gas pressures outside and inside the ring is positive, and less than the pressure of the longitudinal magnetic field:

$$p_e - p_i = \frac{\overline{B_{\parallel}^2}}{8\pi} - \frac{B_{\perp}^2}{8\pi} > 0.$$

— R. LÜST:

This ends the discussion on motion in sunspots, and we change now to the subject of the flare phenomena.

— C. DE JAGER:

I comment not so much on the observations as on their interpretation. Often a flare is pictured as a region where suddenly much heat is released. But I think the most important phenomenon which we observe in a flare is the sudden and large increase in density. Let us picture here the situation. The chromosphere has an electron density of  $10^9$  or  $10^{10}$  particles per  $\text{cm}^3$ . In the corona it is  $10^8$ . The flare arises in a few minutes, and we observe it to have a density of the order of  $10^{13}$ —so you see in a very short time interval the density exceeds that of the surroundings by a great factor. I think this phenomenon to be the most fundamental one of a flare. It is true that a flare may also have a higher (or even lower, depending on whether it is formed in chromosphere or corona) temperature than the surroundings. Different values are quoted in the literature; depending on the way it has been found one gives  $10\,000^\circ$ ,  $50\,000^\circ$ , even  $100\,000^\circ$ . But that is not the main point. I think the essential point is that in a very short time a region of the corona or chromosphere, depending on where it is, collapses to a very high density. A secondary aspect is that such a collapsed region can emit more radiation: the number of particles per  $\text{cm}^3$  is greater, there are more recombinations, etc.

— F. H. CLAUSER:

When you say collapse, this would imply the particles come from outside. Are you sure they don't come from below?

— K. O. KIEPENHEUER:

There is some evidence against this idea, because the structure of the chromosphere underneath the flare doesn't change at all. So there can't be a big flow of mass. I think this is quite crucial.

— ZD. SVESTKA:

I should like to mention one observation which may have a connection with the velocity field in flares. SEVERNY mentioned here this morning that according to the observations made in the Crimea for flares on the limb, the Balmer lines in these flares are broadened by some flare motions. I am not sure that all flares can be interpreted in this way, because many flares can be described also in terms of Stark or damping broadening. But there are several flares quite certainly where this explanation in terms of Stark broadening is not possible because, first, the wings of the Balmer lines do not follow the law of the Stark broadening, second, because these flares are evidently optically thin. We can observe the absorption lines through the emission of the flare. And because we need for the Stark effect a great number of atoms in the line of sight, we must explain the broadening of the lines in these « thin » flares by means of some velocity field. We observed one large flare on the solar surface on July 20, 1958, where we took a series of spectra during the whole development of the flare. This flare is optically thin, and it seems that the lines there had to be broadened by some velocity field. If the Balmer lines were broadened by Doppler effect, then if we plot  $\log \tau$  ( $\tau$  = optical thickness) *vs.*  $(\Delta\lambda)^2$  we should get straight lines. We get such straight lines only just for three minutes in the flash phase of the flare, not for the parts before and after this. We can get such straight lines, however, if we plot  $\log \tau$  *vs.*

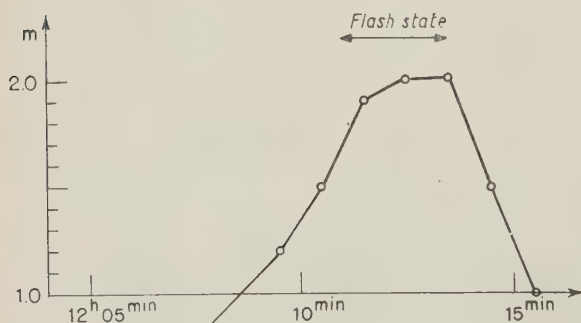


Fig. 4.

$(\Delta\lambda)^m$ , using  $m < 2$ ; this same effect was already observed by JEFFERIES and SMITH at Sacramento Peak. They tried to explain it by non-Maxwellian velocity distributions in flares. But there are some difficulties. The first is that we do not observe corresponding broadening of the lines of calcium and the lines of helium which would

give much smaller velocities than those from hydrogen. And second it is rather curious that just in the flash phase we get these Maxwellian distributions of velocities and not before and after. The value of the quantity  $m$  changes as shown in the figure. There would be another possible explanation; namely, that there appears a change of the Doppler width; namely, of  $\Delta\lambda_D$  inside a flare even if the velocities were Maxwellian. Such a change could give rise to such fictitious straight lines for other values of the power of  $(\Delta\lambda)$ , than 2. Then this graph would mean that this change in  $\Delta\lambda_D$  was very large in the beginning and end of the flare. But, during  $(2 \div 3)$  minutes, there was no change at all. The situation 4 minutes after the maximum of the flare was roughly the same as 4 minutes before. The velocities, of course, were rather large, they were higher than 200 km/s in the flash phase. This cannot be explained as due to temperature, because this would require temperatures of the order of coronal temperatures, more than one million degrees. And, therefore, we are obliged to assume some micro-turbulent motion inside the flare. And I should ask SEVERNY how was the situation in the limb flare he observed at the Crimea; were the  $\text{Ca}^+$  and helium lines inconsistent with the hydrogen lines with respect to velocity or were they not?

— A. B. SEVERNY:

There are some cases in which we observed them. Now we are observing flares with echelle gratings, which permit us to obtain the whole spectrum in the range from 6800 up to 3200 Å, in the form of strips corresponding to the different regions of the spectrum. And sometimes from the observational standpoint it is most important to fix the flare on the slit. And we do really observe sometimes this same picture in the  $\text{Ca}^+$  lines  $H$  and  $K$  and in the helium lines—not only for  $D_3$  of He I but also in  $\lambda 4686$  of He II. We really observed the same wings—very broadened wings as in «mustaches», in these lines simultaneously. But there are some flares in which you can only observe hydrogen emission, but no broad calcium emission and helium emission appear. Sometimes you can observe very strong emission in metallic lines, but emission in hydrogen is comparatively weak.

— A. UNSÖLD:

I was struck by seeing one of the spectrograms taken by SYESTKA, that on this particular spectrum, the metallic lines were quite narrow. How is the situation in your case? Do the metallic lines also show these high velocities of the order of 1000 km/s?

— A. B. SEVERNY:

In some of my cases, the metallic lines are broad.



— A. J. DEUTSCH:

In this Symposium I have heard of many things that I do not understand. But now I wish to inquire about one point that I do not understand more thoroughly than any of the others. This is the equation which governs the time rate of change of the magnetic field in a conducting medium,

$$\frac{\partial \mathbf{H}}{\partial \tau} = \frac{1}{4\pi m\sigma} \nabla^2 \mathbf{H} + \nabla \times (\mathbf{V} \times \mathbf{H}).$$

If one computes the order of magnitude of the right member for the case of a static medium, in order to find the time of rigid free decay for a field comparable in size with a typical flare, he gets, conservatively,  $10^4$  years or more. The observations of SEVERNY indicate that, in the course of a flare, the magnetic field in the reversing layer changes drastically, at least in some parts; and this has also been supported by theoretical arguments advanced, I think, by PARKER. A flare typically releases most of its energy in about 15 minutes. If I did my arithmetic right, the ratio of these two time intervals is of the order of  $10^8$ . Moreover, if fluid motions exist, they cannot accelerate the dissipation. The Laplacian alone gives the dissipative part of the time change of  $\mathbf{H}$ . The curl term dissipates nothing; it just convects the lines of force some place else. How is it done?

— L. BIERMANN:

The problem of how this can be reconciled has been thought of in several steps—and can be found in a paper by SWEET as a contribution to the Stockholm Symposium on Electromagnetic Phenomena in Cosmical Gases. The main point is that this equation has to be applied with some care if mass motion along the lines of force occurs. The second point is that the resistivity in certain layers is very greatly increased by ambipolar diffusion. That plays a great part as has been pointed out on several occasions by SCHLUTER and myself. When you combine all these factors and take into account the special factors introduced by neutral lines in neutral surfaces, you approach an answer. Since at least 1948, it has been recognized that these neutral lines play a serious central role in the discussion of what happens in a flare.

— E. SCHATZMAN:

I would like to say a few words more on the question of the origin of flares associated with neutral points. SEVERNY this morning has already reminded us that when we have on the surface of the sun «hills» of opposite polarity, we have in between these hills a region where the magnetic field vanishes, and that is what is called a neutral point. This is not to be confused with the regions along which the magnetic field, being transverse, is not seen on the

magnetograms. It has been proved already by DUNGEY on one hand, by SWEET on the other that a neutral point is a region of instability, and it was appealing then to try to explain the appearance of the flare at the neutral points by a special kind of instability. For that purpose, I have studied a magnetic field of a much simpler nature—that is, a magnetic field which is periodic in  $x$  and  $y$  and decreases exponentially in  $z$ . Or is constant in the  $z$  direction. I won't draw the picture of the magnetic field, I just want to mention that there is a periodic structure of neutral lines of force—that means neutral lines along which the magnetic fields vanish. The magnetic field which has been used is a so-called force-free magnetic field. We have the advantage that there is no magnetic force so that the pressure equilibrium is realized with a constant pressure and that simplifies the calculations. With that special choice of the magnetic field, I have tried to see whether there was stability or not. It can be seen that there exists perturbations which are unstable if some characteristic value of the magnetic field is greater than a constant times the gas pressure:  $B^2 > P_g \cdot \text{constant}$ . The constant is of the order of unity but has not been found by the theory. So, though I think it is an oversimplified problem, I think it goes in the line of the observation of SEVERNY and worth mentioning briefly here (paper to be published in *Rev. of Mod. Phys.*).

— E. N. PARKER:

I want to call your attention to some of the numbers characterizing solar flares. Let me restrict my remarks to a large solar flare—they come in all smaller sizes so you can scale down my arguments as much as you like. The energy from the large flare (the radiant energy, the visible energy) is not less than  $10^{32}$  erg. LÜST suggested the number  $10^{33}$  erg, and I think that is quite a reasonable estimate. The flare does a number of things, most of these large flares now are observed to emit protons with energies anywhere from 10 MeV up to as high as 30 GeV. Most of them do not emit energies much above 100 MeV, but the total energy in this particle emission is  $10^{31}$  erg, or even  $10^{32}$  erg. Now I might also add that from the gas which blows past the earth a day or so after the flare, you deduce that there must be something of the order of  $10^{18}$  g of matter ejected with a kinetic energy of about  $10^{34}$  erg. Now the first and obvious question is where does the energy of the flare come from. The thermal energy of the entire solar corona is only about  $10^{32}$  or  $10^{33}$  erg. Even if you could bleed the corona on all sides of the sun and feed it into the flare spot, you would probably not have enough energy to run things and, of course, there is no known mechanism for doing this. The observations show that the corona is unchanged during a flare, even fairly close to the flare. People have therefore been forced to the idea, that you have heard frequently expressed today, that the flare energy source must be a magnetic field. Well, a big flare might easily be  $10^4$  km high and it might be 30 000 km on a side—

you find that if this entire volume is filled with a field of 500 gauss and if the onset of the flare completely annihilates that field, then you will have enough energy to perhaps account for the flare. There has been so far suggested no other answer to the riddle of the energy of a solar flare.

— H. LIEPMANN:

It is certain that the energy comes from the flare?

— E. N. PARKER:

No, but the flare is what is making all the noises and waving all the flags and one assumes that it is the center of the energetics. There is no other disturbance that can be seen on the sun at the time of the flare, so it would be even more mysterious if the energy came from a quieter region.

— H. LIEPMANN:

No, but it could be the same cause that causes the flare to produce itself.

— E. N. PARKER:

You are correct. The visible energy is in fact perhaps only a small portion of the total energy, and therefore might be a decoy.

— I. K. CSADA:

I would like to comment on some statistical evidence for the general magnetic field of the sun.

In the following a statistical method will be proposed for evaluation of Babcock's magnetograms in order to study the structure of the general magnetic field of the sun. As the local fields show random fluctuations and suggest the existence of magneto-hydrodynamic turbulence the usual representation for the local field is as follows

$$H = \bar{H} + H',$$

where the mean value  $\bar{H}$  may be considered as the general field and may be supposed to be governed by the differential equation deduced for the averages.

In the magnetograms the component of the magnetic field in the line of sight is recorded *i.e.* we may write

$$H_x = \bar{H}_x + H'_x$$

and the determination of  $\bar{H}_x$  may be carried out planimetrically. As it seems theoretically possible to suppose that the symmetry of the magnetic field is

axial (magnetic axis) the field may be represented by the vector potential

$$A_{\varphi} = \sum h_n \frac{P_n^{(1)} \cos \psi}{r^{n+1}},$$

where  $\psi$  is the polar distance related to the north magnetic pole. Simple deduction show that the  $x$  component of the mean value of the magnetic field along constant  $\theta$  is

$$H_x = \sum \frac{2nh_n}{r^{n+2}} P_n(\cos q) P_{n+1}^{(1)}(\cos \theta),$$

where  $q$  denotes the distance of the magnetic axis from the axis  $Z$ .

Let us introduce the following notation

$$A_n = \frac{2h_n}{r^{n+2}} P_n(\cos q)$$

then the magnetic strength on the solar surface is

$$\bar{H}_x = \sum A_n P_{n+1}^{(1)}(\cos \theta)$$

which will be considered as an interpolation formula.

This expression will be used for the model of a two-term potential function (DO model) in the following form

$$\bar{H}_x = A_1 P_2^{(1)}(\cos \theta) + A_3 P_4^{(1)}(\cos \theta).$$

As the calibration factor is not given for all magnetograms, we must reduce the analysis to the non-dimensional parameter  $h = A_3/3A_1$  being independent of the calibration, that is

$$c\bar{H}_x = P_2^{(1)}(\cos \theta) + 3hP_4^{(1)}(\cos \theta),$$

where

$$h = \frac{h_3}{h_1} \frac{1}{r_0^2} \cdot \frac{1}{2} (5 \cos^2 q - 3).$$

In the statistical analysis  $h$  was determined for 250 disc recordings (magnetograms) which were made in the Mount Wilson Observatory and were sent by BABCOCK to the University of Szeged. Values of  $h$  were found between 0.0 and 1.0, but a well defined grouping appears at 0.4. The explanation of the spread is as follows:



i) It is possible that the structure of the magnetic field characterized by  $h_3/h_1$  would be a random function of the time and we can mention «general field» in the statistical sense.

ii) From the statistical point of view  $h_3/h_1$  may be nearly constant in time (its random fluctuations are very small), but the orientation of the magnetic axis varies in the system  $XYZ$ .

A periodical variation of  $h$  seems to appear from time to time synchronously with the synodic rotation of the Sun. This fact suggests a deviation of the magnetic axis from the rotational axis. As  $q$  is given by the expression

$$\cos q = \cos s \cos v + \sin s \sin v \cos \omega(t - t_0)$$

(where  $s$  is the distance of the axis of rotation from  $Z$  and  $v$  that of the magnetic axis from the axis of rotation) it is clear that  $q$  and also  $h$  must show two periods: annual period and rotational period. Both periods are found and

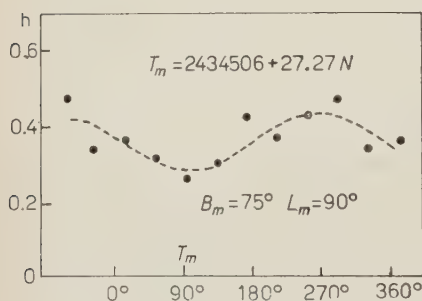


Fig. 5.

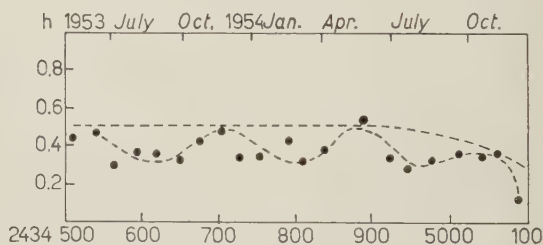


Fig. 6.

shown in Fig. 5 and 6. From the rotational period it was possible to estimate the co-ordinates of the north magnetic pole

$$B_m = 75^\circ \quad \text{and} \quad L_m = 90^\circ.$$

Now, I shall show some consequences of these results relating to the solar atmospheric phenomena.

The disturbances which are generated in the photosphere propagate through the chromosphere into the corona as Alfvén waves (magnetohydrodynamic waves) and magneto-acoustic waves.

It is possible to limit areas in the field of the DO model in which no Alfvén waves can proceed radially. From this point of view the discussion like the one published by BILLINGS at the High Altitude Observatory seems to be important. This paper contains observations of some wave motions in the

corona, and the anomaly is pointed out at  $B = 50^\circ$  where no Alfvén waves can proceed in DO model.

Finally, I should like to mention that the orientation of the spiculae seems to follow the lines of force of the DO model better than that of a simple dipole field. Fig. 7 shows the orientation of the spiculae (derived by LIPPINCOTT, published in the *Contribution of the Smithsonian Institution*) and the line of force of the DO model at  $h = 0.4$ . I think such an interpretation of spiculae may be important to determine the solar magnetic field during the maximum activity when Babcock-magnetograms are not evaluable in this way as the local fields are too large compared to the general field.

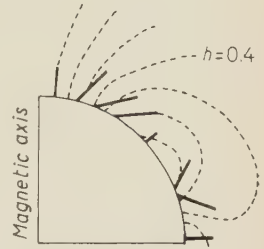


Fig. 7.

## PART IV.

# Considerations on Localized Velocity Fields in Stellar Atmospheres: Prototype — The Solar Atmosphere.

## D. - Collision-Free Shock-Waves.

### Summary-Introduction: Collision-Free Plasmas (\*).

H. E. PETSCHER

*Avco-Everett Research Laboratory - Everett, Mass.*

#### 1. - Introduction.

In many cases, the outer atmosphere or corona regions of stars are not dominated by the ordinary process of collisions between ions. Very roughly, collision-free phenomena become of interest when the mean free path for interparticle collisions is larger than the region of space being considered. For solar corona conditions, if we consider a density of  $10^7$  particles per cubic centimeter and a typical particle velocity of 500 kilometer per second, the mean free path is a few times the solar diameter. At the same velocity for a density of 100 particles per cubic centimeter, corresponding to the interplanetary density, the mean free path would be a thousand astronomical units. Since in both of these regions, we are interested in smaller dimensions, it is of interest to consider the properties of a plasma in which collisions are very rare.

A somewhat more precise definition of the region where collisions are unimportant, is obtained by comparing the mean free path to the ion gyro radius. When the mean free path is the larger of these quantities, some important changes in the plasma properties which may be described as collision-free plasma effects become important. A more detailed discussion of the boundaries of such regions was given at the previous Symposium [1]. According to this criterion of gyro radius and mean free path, the photosphere of the sun is definitely collision-dominated, whereas, the solar corona is essentially collision-free.

In this paper, we will consider two aspects of collision-free plasmas—the structure of a shock-wave moving through such a plasma, and the electrical conductivity of such a plasma. As we shall see, the thickness of a shock-wave may be appreciably thinner than the mean free path for interparticle collisions.

---

(\*) This paper is a summary of work done largely under Contracts no. 2524(00) and AF 49(638)-61.

sions and therefore, there may be appreciably more abrupt changes in the state of the plasma than one might otherwise have anticipated. The electrical conductivity can be appreciably reduced, as compared to the standard value derived from interparticle collisions, due to the presence of appreciable wave motion in the plasma. This reduction may in some cases be sufficient so that the usual astronomical assumption of the fluid being tied to the magnetic field lines will not always be valid.

## 2. - Wave turbulence.

One of the striking features which has been discovered in laboratory plasma experiments is that a phenomenon similar to turbulence frequently occurs in collision-free plasmas [2]. Large scale fluctuations in plasma properties appear and considerably increase the rate of dissipation in the plasma. The understanding of this phenomenon seems to be fundamental to a discussion of the transport properties of such a plasma. The use of the term turbulence should not be taken to imply that the mechanism is as complex as it is for ordinary aerodynamic turbulence. As we shall see, these fluctuations in plasmas have a much different physical basis and are easier to understand.

If we examine the small amplitude perturbations which can exist in an otherwise uniform collision-free plasma, we find that most such disturbances can be described as *propagating* waves. The exceptions to this are the very restricted class of disturbances that are completely independent of the co-ordinate along the magnetic field lines. The basic elements which make up plasma turbulence are, therefore, probably propagating waves rather than disturbances which remain stationary in the fluid.

Plasma turbulence may be considered as a distribution of randomly oriented waves propagating through the plasma. As these waves propagate they carry momentum and energy from one place to another. Due to the non-linear interactions between these waves, a particular wave has a finite lifetime at the end of which its momentum and energy has been transferred to other waves. If we visualize the waves as being grouped into wave packets, this picture is closely analogous to the kinetic theory picture of an ordinary gas. The wave packets replace the atoms, and the distance travelled by a wave in its lifetime replaces the mean free path.

The waves which will be of interest to us for the two problems which are considered here, are the magnetohydrodynamic waves at frequencies in the neighborhood of the ion cyclotron and somewhat higher. For these waves, the mean free path has been estimated [3] by considering the random walk of the phase of a wave as it moves through the disturbances in the plasma properties caused by the other waves present in the plasma. The resulting



mean free path was

$$(1) \quad \lambda = \frac{4}{\beta \bar{k}},$$

where  $\bar{k}$  is the mean wave number in the distribution of waves and  $\beta$  is the ratio of wave energy to the average magnetic field energy. This result may be interpreted simply as a statement that the scale is determined by the mean wave length of the turbulence and that the number of wavelengths which a wave moves between collisions is inversely proportional to the amount of disturbing wave energy present.

Before developing the consequences of this kinetic theory picture of wave turbulence, it is of interest to compare this picture with the picture of ordinary aerodynamic turbulence. In the latter case, the fundamental element is a vortex which does not propagate through the fluid in the absence of other disturbances. This means that fluid properties are transported from one place to another only through the non-linear interactions of different vortices. The simple picture of an individual element of the turbulence transporting fluid properties is therefore not valid. Furthermore, the fact that vortices do not propagate means that vortices retain the same neighbors, and a high degree of correlation results which must be properly taken into account. In a plasma, waves attain new neighbors due to their propagation, and the simplifying assumption that interacting waves have random phases is justified.

### 3. - Shock structure.

Before discussing the structure of a shock-wave in a collision-free plasma, we will review very briefly some of the gross features of shock-waves in a neutral gas where collisions dominate. The first point to be made is that a pressure pulse whose initial dimensions are large compared to the mean free path will tend to steepen to form a shock front. In other words, if viscous forces and heat conduction may be neglected, such a pressure pulse will tend to steepen towards a discontinuity. When such a discontinuity is reached, it is apparent that the neglect of dissipative effects is no longer valid. If one is interested in the large scale properties of the flow, one may calculate the conditions across the discontinuity (shock-wave) from the laws of conservation of mass, energy, and momentum. If, on the other hand, one is interested in resolving the discontinuity, one must calculate the local region of the flow including the effects of viscosity and heat conduction.

A very rough estimate of the resulting thickness of a shock-wave may be obtained in the following manner. In the absence of viscosity, the pressure rise is unbalanced for a steady flow as indicated by the fact that pulse steep-

ening occurs. In order to maintain a steady shock structure, this excess pressure must be balanced by viscous stresses. For a strong shock wave, the magnitude of the unbalance is of the order of the gas pressure. If the viscous stresses are to be this large, the distribution function of the particles must be appreciably distorted from a Maxwellian. In order to achieve this, there must be appreciable gradients per mean free path. The resulting shock thickness is therefore of the order of a mean free path. More detailed considerations, both experimental and theoretical, show that the shock thickness for a strong shock in an ordinary gas is of the order of two mean free paths in the gas ahead of the shock [4].

If we now consider the case of a plasma (in the absence of turbulence) in which the mean free path is appreciably longer than the ion gyro radius, the viscosity is reduced by the square of this ratio since the ions are no longer free to move with a constant velocity between collisions. This means that the viscous forces are appreciably reduced and cannot balance the excess pressure when the pulse thickness is of the order of the mean free path. The steepening therefore proceeds to a thickness appreciably less than the mean free path. In this case, very few collisions between particles will occur inside the shock front, and we may consider interparticle collisions to be completely absent in the shock front.

If we now want to consider the structure of such a shock-wave from the point of view of kinetic theory of waves, we must first show that large wave amplitudes can be built up by the shock wave [5]. This can be done by considering a small amplitude, small scale disturbance superposed on the velocity gradient which is present in a shock-wave. This small disturbance or wave packet will exert a pressure on the fluid. In the presence of a compressive velocity gradient, there will be a net amount of work done on the wave packet by the flow, and the energy of the wave packet will increase. This compression work is the source of wave energy in the shock front. This mechanism will be most effective for those waves which can spend a long time in the compressive region of the shock. Thus, waves with a group velocity comparable to the flow velocity will predominate. For shock-waves moving perpendicular to the magnetic field at shock speeds not too much above the Alfvén speed, the waves which will be selected on this criterion are the magnetohydrodynamic waves at frequencies somewhat above the ion cyclotron frequency.

As in the case of an ordinary gas, we must have an appreciable distortion of the distribution of waves to give appreciable viscous stress and heat conduction. This implies that the shock thickness will again be a few mean free paths. In the present case, of course, it is a few mean free paths for the waves. The mean free path for the waves as given by eq. (1) depends on the wave energy as well as the mean wave number. The mean wave number is determined from the above criterion of the appropriate wave velocity. The wave energy

is determined from the conservation equations. All of the energy which would normally be thermal energy behind the shock is wave energy in this case. The

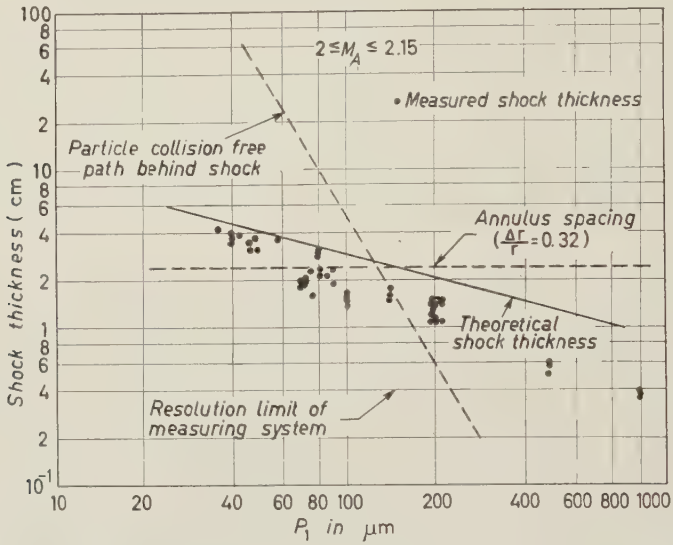


Fig. 1. — Dependence of shock thickness on density. At low densities, the shock thickness is less than the interparticle collision-free path and agrees with the predicted collision-free shock thickness. At high densities, the mean free path is reduced and one would expect a shock thickness several times the interparticle mean free path.

resulting estimate of shock thickness and its dependence on density and Alfvén-Mach number [6] are given in Fig. 1 and 2.

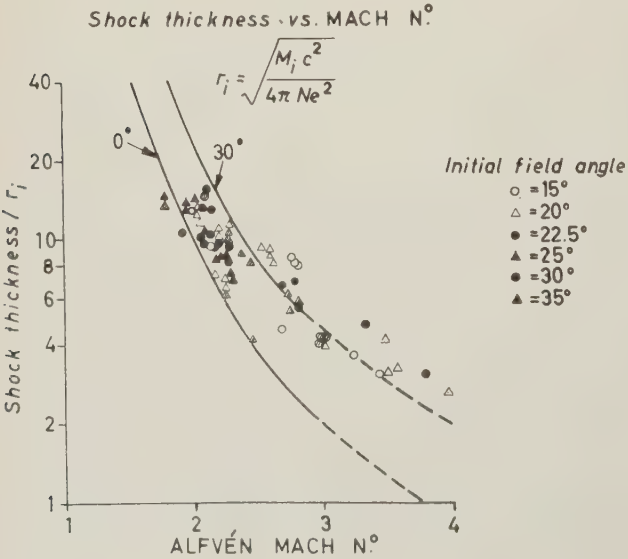


Fig. 2. — Dependence of shock thickness on the ratio of shock speed to Alfvén speed ahead of the shock. With increasing Alfvén Mach number the shock thickness decreases roughly as predicted. The ordinary particle collision shock thickness would increase. The angle referred to is the angle between the magnetic field ahead of the shock and the plane of the shock. Over the range covered, no experimental dependence is observed. The theoretical variation is, however, also rather small.

The experimental data shown in these figures was obtained by PATRICK [7] in a magnetic annular shock tube. This is a device in which the test gas occupies the annular space between two concentric cylinders. Current from a condenser discharge between the inner and the outer cylinders creates a magnetic field which acts as a piston. This piston pushes the gas down the annulus to form a shock-wave. These shock waves were observed principally by recording the emitted bremsstrahlung from the hot gas with a collimated photomultiplier system looking across the annulus.

Shock speeds of the order of 300 km per second were obtained at the densities used. The absolute value of the radiated intensity varies as the square of the electron density and is almost independent of the temperature. Its measurement can therefore be used to determine the density behind the shock-wave. In Fig. 3, a comparison is given of the theoretical and experimental light intensities which are seen to be in very good agreement. This agreement shows that the interactions with the wall do not appreciably change the conservation relations across the shock. That is, the effects of the wall are presumably confined to a narrow region near the wall.

The experimental shock thicknesses were obtained by measuring the rise time of the light intensity as the shock passes the collimating slits. The resulting agreement between theory and experiment is somewhat better than one might expect from the present crude form of the theory.

The theoretical shock thickness at a fixed Alfvén-Mach number varies inversely as the square root of the density. For a shock speed of twice the Alfvén speed and a density of 100 particles per cubic centimeter corresponding to the interplanetary gas, the predicted thickness is still only 200 km.

This scale is still reasonably small compared to the length scales which are of interest. We may therefore conclude that shock waves of almost negligible thickness can exist in the interplanetary medium in spite of the extremely long mean free path for interparticle collisions.

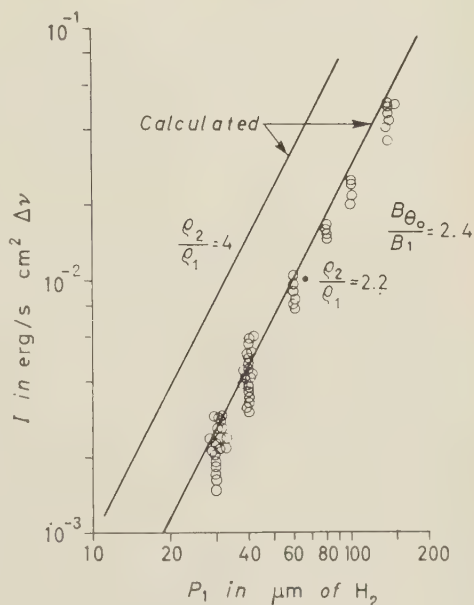


Fig. 3. — Measured light intensity behind the shock wave as a function of density. The expected value for the initial magnetic field used corresponds to  $\rho_2/\rho_1 = 2.2$



Another type of approach to the collision-free shock which has been attempted in several variations is to assume that the flow is completely one-dimensional—that is, independent of the co-ordinates in the plane of the shock front. This approach is somewhat questionable in view of the argument for the growth of wave energy which was given. This argument would imply that such solutions might be unstable to three-dimensional disturbances. These treatments will not be considered in this paper since some of the proponents of these theories are present and will bring them up in the discussion.

#### 4. – Electrical conductivity.

The suggestion that small-scale turbulence could be responsible for an enhanced diffusion of plasma across a magnetic field was first made by BÖHM [8]. BUNEMAN [9] considered the reduction in electrical conductivity associated with the build-up of waves by the two-stream instability. His mechanism however, requires currents corresponding to electron velocities greater than the electron thermal velocities. Such large values of current will not occur in general. SPITZER [10] has considered the random walk diffusion of particles due to the presence of a wave field. His results, however, would seem to be applicable only to the small number of particles which move in phase with the waves. His diffusion rate applied to the plasma as a whole is, therefore, probably a gross overestimate.

In an ordinary plasma in which collisions dominate, the electrical conductivity is determined by the friction force which results from collisions between electrons and ions when the electron stream has a velocity relative to the ions. If the electrical conductivity is to be reduced by the presence of wave turbulence, the waves must be more effective in producing such a friction force than the interparticle collisions are. In order to see how a wave field could give rise to such a force, let us assume that a wave field exists such that the electric field associated with the waves has a component which is in phase with the fluctuations in the mass density. A net force would result on the relative motion between the electrons and ions, since in the high density regions there are more electrons than ions present to be accelerated in one direction than are present in the low density region where the acceleration is in the opposite direction. A similar term would also arise if there were a correlation between the mass velocity of the fluid and the perturbed magnetic field. If we consider both of these terms, the momentum balance for the electrons for a quasi-steady state current may be written as

$$(2) \quad \varrho_0 E_0 = - \overline{\varrho' E'} - \varrho_0 v' \times B',$$

where  $\rho$  is the mass density of the plasma,  $E$  the electric field,  $v$  the mass velocity, and  $B$  the magnetic field, the subscript 0 indicates the undisturbed conditions, the prime indicates quantities which fluctuate due to the turbulence and the bar indicates an average over a region large compared to the wavelength of the turbulence. The left-hand side indicates the acceleration due to the applied static electric field and the right-hand side represents the friction associated with the fluctuating fields. It is important to note that this equation suggests that very high frequency waves which oscillate too rapidly to produce changes in the mass density will not be effective in producing the required friction force.

The friction force resulting from a linear wave of constant amplitude is zero, since a wave which proceeds unchanged cannot continually supply momentum to the relative motion between electrons and ions. This may be regarded as the statement that the effective electric field and the density are exactly 90 degrees out of phase for a wave of constant amplitude. If, however, one considers a wave whose amplitude is changing with time, the phase angle between these quantities will change slightly and an in-phase component will appear. There is then a net force associated with a wave whose amplitude is changing with time. The order of magnitude of this force has been estimated by considering the linear dispersion relation for a zero temperature plasma in which the wave is artificially excited by the presence of a hypothetical oscillating body force. For the case of the magnetohydrodynamic waves at frequencies much smaller than the electron cyclotron frequency the friction force is of the order of magnitude of the rate of change of momentum of the wave (rate of change of energy divided by phase velocity).

Unless strong damping or strong instabilities exist, the time rate of change of the energy of a particular wave is probably determined by collisions with other waves. Roughly speaking, the friction force resulting from a collision between waves will be of the order of magnitude of the momentum interchange in the collision.

If the waves present in the plasma have a symmetric distribution of the wave vectors, the net friction force resulting from each of the individual collisions will average to zero. In order to obtain a net friction force when averaged over all the collisions, there must be an asymmetry in the distribution of waves. This asymmetry results from the presence of a current in the plasma. Depending upon the frequency of the wave, the wave may be more coupled to the electron or to the ions. The waves coupled to the ions will tend to move with respect to a co-ordinate system determined by the ions, whereas, those coupled to the electrons move with respect to a co-ordinate system determined by the electron motion. In other words, one would expect that a change in the phase velocity of some waves would be associated with the introduction of a current into the system. If one calculates the dispersion relation for the

magnetohydrodynamic waves in a zero temperature plasma and in the presence of a steady current, one does find a change in the phase velocity of the waves which is of the order of magnitude of electron velocity associated with the current. This change in phase velocity is a function of the frequency of the wave. It is constant at very low frequencies and at very high frequencies, and it changes in the neighborhood of the ion cyclotron frequency. In the presence of a current, therefore, the high frequency waves will have a net drift velocity relative to the low frequency waves. This gives rise to an asymmetry in the distribution of waves which is proportional to the current. The collisions between the high and low frequency waves result in a transfer of momentum between the two groups of waves. This momentum transfer between the waves is transmitted as a momentum transfer between electrons and ions by the mechanism discussed earlier.

Since the net friction is proportional to the current, we have a linear relation between the current and the electric field. The order of magnitude of the resulting conductivity can be estimated from the above arguments using eq. (1) to determine the mean free time for collisions between waves and taking the mean frequency of the wave distribution to be the ion cyclotron frequency. This choice of mean frequency is dictated by the fact that it separates the regions of different change of the phase velocity of the waves. Waves must be present on both sides of the ion cyclotron frequency in order to have a relative velocity between them.

The resulting conductivity is

$$(3) \quad \sigma = \frac{8Nec}{B} \frac{1}{\beta^2},$$

where  $N$  is the particle density,  $B$  is the average magnetic field,  $e$  is the electronic charge, and  $c$  the velocity of light. The ratio of this conductivity to the conductivity determined by the interparticle collisions is  $8\beta^2/\omega_{ce}\tau_e$ , where  $\omega_{ce}$  is the electron cyclotron frequency and  $\tau_e$  the electron mean free time. Since  $\omega_{ce}\tau_e$  can be very large under high temperature and low density conditions, the reduction in conductivity can be appreciable even for moderate values of  $\beta$  [11].

A very rough comparison of this theory with experiment may be obtained from some stabilized pinch experiments of COLGATE [2]. The measured lifetime of the pinch indicates a conductivity of  $10^3$  mho per cm at a density of  $2 \cdot 10^{15}/\text{cm}^3$  and a magnetic field strength of  $2 \cdot 10^4$  gauss. No direct measure of  $\beta$  exists for these experiments. However, the plasma pressure was estimated for magnetic probe measurements to be about one-sixth of the magnetic pressure. If the arbitrary assumption is made that all of this pressure is wave pressure, the predicted conductivity is  $5 \cdot 10^2$  mho per cm.

The astronomical implications of this reduced conductivity are not completely clear since the magnitude of  $\beta$  which occurs is generally not known. In principle, of course, this is calculable from an energy balance, consideration of the wave energy. The joule dissipation associated with these waves increases the wave energy and damping of the waves, as well as their diffusion out of the current region decreases their energy. This energy balance has, however, as yet not been considered even for the simpler laboratory cases. It is, however, interesting to note that if a large value of  $\beta$  does occur, the electrical conductivity can become so low that in some astronomical cases, appreciable diffusion of the magnetic field relative to the plasma can occur. For example, for  $\beta=1$ , a density of  $10^8$  particles per cubic centimeter, and a magnetic field strength of  $10^3$  gauss, the diffusion depth is of the order of  $3 \cdot 10^{10}$  cm in one month. This implies that for sufficiently large  $\beta$ , the magnetic field above a sunspot can diffuse across the dimension of a sunspot in a time less than the lifetime of the sunspot. These arguments, of course, do not apply to the currents in the photosphere and below, which are responsible for the magnetic energy of the sunspot. The structure of the field in the corona above the sunspot is, however, of interest in determining the behavior of disturbances. This structure may be appreciably influenced by the diffusion mechanism described above.

## 5. - Summary.

In the corona regions of stars the ratio of the mean free path for inter-particle collision to the gyro radius becomes very large. Under these conditions the transport processes in the plasma become dominated by plasma turbulence. This phenomenon can be described in terms of a kinetic theory of a random distribution of waves. The structure of a shock-wave under these conditions has been considered. Even at densities as low as  $10^2$  particles per  $\text{cm}^3$  the thickness of such a shock-wave should be very thin, less than 100 km. The effects of such a turbulent wave field on reducing the electrical conductivity has also been considered. It seems possible that appreciable diffusion of the magnetic field relative to the plasma could occur under some astronomical conditions.

## *Note added in proof.*

Recent more detailed analysis of the shock structure problem (to be published in *Proc. of the Intern. Atomic Energy Conf. on Plasma Physics and Controlled Nuclear Fusion Research* to be held in Salzburg, Austria, from 4 to 9 September 1961) has indicated that in order to persist the wave distribution must be highly asymmetric. This casts some doubt on the assumption in the estimate of electrical conductivity that a small departure from a symmetric distribution is caused by the current.



## REFERENCES

- [1] PETSCHKE, H. E., *Rev. Mod. Phys.*, **30**, 966 (1958).
- [2] COLGATE, S. A., FERGUSON, J. P. and FURTH, H. P., *Proceedings of the Second International Conference on Peaceful Uses of Atomic Energy*, **32**, 129 (1958).
- [3] FISHMAN, F. J., KANTROWITZ, A. R. and PETSCHKE, H. E., *Rev. Mod. Phys.* **32**, 959 (Oct. 1960).
- [4] SHERMAN, F. S., NACA TN 3298, July 1955; MOTT-SMITH, H. M., *Phys. Rev.*, **82**, 885 (1951).
- [5] The suggestion that instabilities would be important in a shock wave in the absence of a magnetic field was first made by F. D. KAHN (*Gas Dynamics of Cosmic Clouds*, New York, 1955, chap. 20, p. 115) and discussed somewhat further by E. N. PARKER: *Astrophys. J.*, **129**, 217 (1959).
- [6] A more detailed account of this theory is found in ref. [3].
- [7] PATRICK, R. M., *Phys. of Fluids*, **2**, 589 (1959) and **3**, 321 (1960).
- [8] BÖHM, K., in *The Characteristics of Electrical Discharges in Magnetic Fields*, edited by A. GUTHRIE and M. K. WAKERLING (New York, 1949), chap. 2, Sect. 5.
- [9] BUNEMAN, O., *Phys. Rev.*, **115**, 503 (1959).
- [10] SPITZER, L., *Phys. of Fluids*, **3**, 659 (1960).
- [11] A more detailed account of this theory is presently being prepared for publication.

## PART IV.

# Considerations on Localized Velocity Fields in Stellar Atmospheres: Prototype — The Solar Atmosphere.

## D. - Collision-Free Shock-Waves.

### Second Summary-Introduction:

### Steady One-Dimensional Fluid-Magnetic Collisionless Shock Theory (\*).

A. A. BLANK and H. GRAD

*Institute of Mathematical Sciences, New York University - New York*

### 1. — Introduction.

Shock-waves represent one of the most important mechanisms for creating and heating a plasma. In classical non-dissipative gas dynamics, the formation of a shock is indicated by the progressive steepening of a finite-amplitude compressive wave front to the point where it becomes multivalued and consequently without physical meaning. This difficulty is avoided by the inclusion of dissipative effects, usually in the form of heat flow and viscosity. The dissipative mechanisms become more effective as the wave front steepens, and the result is a steady wave profile for which the non-linear and dissipative effects are counterbalanced. The scale length for the dissipative transition zone or wave profile is the mean-free-path; the actual thickness may range from one to several mean-free-paths, or even more for very weak shocks. Given the strength of the shock, the state on one side of the shock may be computed from the state on the other side directly from the laws of conservation of mass, momentum and energy (Hugoniot relations). Accordingly, the nature of the particular dissipative mechanism affects only the shape of the shock profile but not the end states.

In a plasma without magnetic field, the conventional theory yields essentially the same results. The inclusion of the additional dissipative mechanism of electrical resistivity again results in a thickness of the order of a mean-free-path or larger. The presence of a magnetic field complicates matters considerably, but the essential features are not changed.

The interesting problems are concerned with high-temperature plasmas

(\*) The work presented in this paper is supported by the U. S. Atomic Energy Commission under contract AT(30-1)-1480.

where the mean-free-path is often extremely large compared to the scale of observed phenomena (collisionless plasmas). In such a case we could not observe a shock-wave attributable to conventional dissipative mechanisms. Fortunately the conventional theory (that is, the conventional modification of the stresses and heat flow in a magnetic field) is inapplicable when the gyro-radius is smaller than the mean-free-path. When the mean-free-path is eliminated as a significant reference length, we might naturally expect the Debye length or the gyro-radius to take its place. The significant length parameter (at least for a certain range of shock strengths) actually turns out to be the speed of light divided by the plasma frequency, a value which is roughly intermediate between the ion and electron gyro-radii. The problem is to exhibit an irreversible mechanism which is effective at this scale of length. In this connection it is crucially important to recognize that irreversibility has not been eliminated by removing the collision term in the Boltzmann equation, but merely concealed. We shall see in Section 2 how the fundamental irreversible mechanism described by GIBBS may still be relied upon.

In addition to the problem of demonstrating the existence of shocks on a scale much smaller than the mean-free-path, it is necessary to solve the complete shock transition problem in a high temperature plasma explicitly even to find the correct relations between the constant states on either side of the shock transition. Specifically, in the absence of collisions, there is no reason why the state after the shock should be in thermal equilibrium: the ion and electron temperatures can be different. Conservation of mass, momentum and energy can only predict the mean temperature. It is this feature which requires study of the entire transition problem in order to even compute the end state (which is obtained in a conventional shock, by use of the conservation equations alone). On the other hand, a single extra piece of information *e.g.*, that the electron orbits are approximately adiabatic, serves to determine the end states uniquely, cf. [1]. For some purposes, knowledge of the oscillating fine structure within a shock may be valuable without detailed knowledge of the ultimate damping length. This, we shall see, is a simpler problem.

The theories summarized here are steady and one-dimensional. This is in contrast to other «collisionless» shock theories [2] which require the intervention of turbulent three-dimensional non-steady waves to effect the transition from one state to another. The accessory hypotheses of the two types of theory are incompatible (although it is conceivable that each could be correct in a different parameter range). The basic premise of the steady theory is that the solution (which has only been obtained approximately, thus far) is stable. The fluctuating theory is based on a number of *ad hoc* premises of which the most prominent is the existence of random waves of exactly the correct amplitude to provide the requisite interaction. Neither theory is more than semiquantitative as yet in predicting a shock thickness. The steady-

state theory is farther advanced in examining the fine structure of a possible shock. There is no experiment which can yet be interpreted as yielding an unambiguous collisionless shock thickness.

## 2. — The irreversible mechanism (\*).

We consider a one-dimensional steady problem in which the fluid flow is taken parallel to the axis of the space variable. The shock configuration is one in which the (uni-directional) magnetic field is perpendicular to the fluid flow. To be properly called a shock the solution should connect a constant state at infinity on one side with another constant state at infinity on the other. Such a transition is necessarily irreversible as a direct consequence of the conservation laws. The problem is to exhibit a mechanism for irreversibility in the absence of collisions.

Irreversibility arises in a dynamical system from a loss of information, specifically of initial order. A system appears to be irreversible when states which are initially close together subsequently become arbitrarily far apart. Thus a fully determinate system may appear to behave randomly when observed on a macroscopic scale. Mathematically, this situation may be described by saying that the theorem of continuous dependence on the initial state becomes increasingly irrelevant as time progresses. The classical mechanism of intermolecular collisions is particularly efficient in destroying initial order. A slight change in the initial position or velocity of a molecule can make it miss or hit another molecule, and hence affect its subsequent history grossly. Although this is a very efficient mechanism, it is by no means necessary to rely on such essentially discrete encounters to obtain irreversible behavior. In « Landau » damping, the essential irreversible mechanism lies in the fact that a slight deviation in initial velocity can produce a large deviation in position at some later time. The present shock problem is more subtle since it can be proved that two particles which start close together will never become separated by more than a bounded distance, no matter what their velocities are. Nevertheless, in traversing the shock, two particles which are originally close in both position and velocity can suffer entirely different histories. For example, some ions will go through the potential barrier represented by the first crest of an oscillating shock front, while others may be reflected on the first approach and only penetrate the second time. Since the time required

(\*) This mechanism was proposed in [1] as a substitute for Landau's damping which disappears for waves propagating perpendicular to  $B$ . Both are special cases of Gibbs' irreversibility mechanism.



for the return of a reflected ion is large compared to the transit time through the crest, this distinction represents a radical difference in history of the two types of ion. Specifically, a circle in velocity space (Fig. 1a) on which the distribution function is constant at minus infinity (it is assumed to be isotropic) will develop

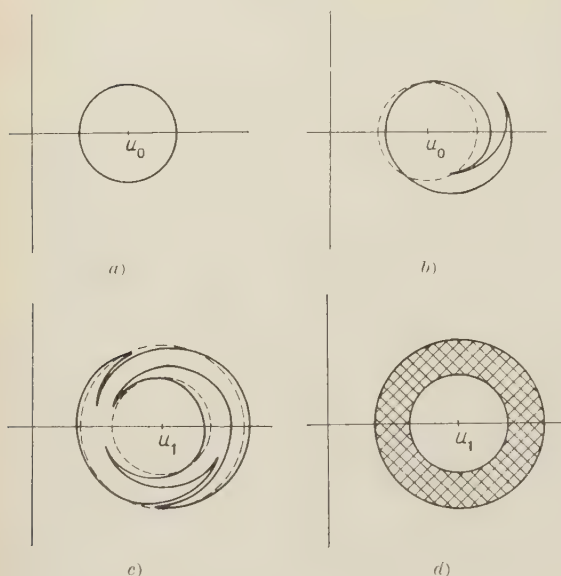


Fig. 1.

an « ear » or extreme distortion after passage through the first wave of the electromagnetic disturbance (Fig. 1b). Presumably, on passing through the successive potential barriers of an approximately periodic wave train, more and more ears will develop, (Fig. 1c) and the distribution function will ultimately become very wild and converge weakly on a different isotropic state at a higher entropy (Fig. 1d). In other words any macroscopic or averaged property will behave irreversibly. What has been confirmed

by a combination of analysis and numerical computation is the development of the first ear and the concomitant partial irreversible shock transition (see Section 7).

### 3. - One-fluid theory.

If the equations of mass and momentum of the total fluid (ions and electrons) are supplemented by the *ad hoc* assumptions of an adiabatic equation of state and Ohm's law for a perfect conductor, the system is mathematically equivalent to that of a conventional gas flow with adiabatic exponent  $\gamma = 2$ . The steady-flow problem has as solutions either the constant state throughout (no shock) or a discontinuous front separating two constant end states. The gas-dynamical analogue holds even for the time-dependent problem [3] and thus yields familiar results such as the steepening of a compression wave, the broadening of a rarefaction wave, and the distortion of a symmetric compressive pulse, first as the forward side steepens into a shock, second as the broadening rarefaction tail eats into and weakens the shock [4].

#### 4. — Two-fluid adiabatic theory.

In this theory we adopt an *ad hoc* adiabatic equation of state for both electrons and ions and use both sets of momentum equations, thereby eliminating the necessity of a special assumption regarding Ohm's law [1, 5, 6]. No shock is obtained. But, in contrast to the one-fluid theory, non-trivial steady one-dimensional flows are found. There is a family of periodic waves and, as a limiting case, a solitary wave or pulse in which both end states are identical. The length scale in these solutions is given by the ratio of the speed of light to the plasma frequency,

$$L = \sqrt{m_-/\mu_0 n e^2},$$

and the actual wavelength is increased above this by a factor of the order of  $1/\sqrt{M^2-1}$  for weak shocks ( $M$  is the Mach number).

We can use these solutions to indicate the region of validity of the more primitive one-fluid theory and to project to a possibly more exact theory. For the former, one can verify that Ohm's law is satisfied (yielding the simpler theory) when the gradients are smaller than those of the stationary pulse solution for a given amplitude disturbance. Thus, a pulse which is wide (considering its amplitude) will start to steepen on one side and flatten on the other. The simple theory breaks down after the wave steepens sufficiently, and the subsequent behavior is complex. One cannot conclude anything about the stability of the two-fluid pulse from this one-fluid analysis, except that it is clear that an initial pulse which starts sufficiently far from the steady two-fluid pulse solution will not converge to it.

As to predictions regarding more accurate theories, one could guess that the periodic solutions are plausible indications of a fine structure within a shock. One should keep in mind the possibility of a smooth transfer from the start of a pulse to an approximately periodic wave of diminishing amplitude converging on the final state behind the shock [1].

To justify the use of adiabatic relations, one should restrict the parameters to guarantee that both the ion and electron orbits are approximated by the guiding center theory. This requires that a Larmor period be completed while the field seen by a particle varies only little, and this, in turn, is easily seen to require that the shock be weak,  $M \sim 1$ . This is an unfortunate restriction. We return to this point later.

#### 5. — Refined two-fluid theories.

A number of fluid-like theories have been attempted using approximations to the stress tensor suggested by moment equations resulting from the exact

particle equations [6-9]. Thus far, the only solutions obtained are periodic waves or some modification of a pulse. For example, in [6] and [8] «pulses» are found with more than one peak. This might be interpreted as a tendency to transfer from the pulse to a periodic solution. An argument is given in [7] based on even and odd behavior of the variables, which argument can probably be extended to an arbitrary moment approximation to the particle distribution function. In brief, if a compressive shock solution exists, then the equations must also admit the mirror-image rarefaction shock. What is more likely is that no shock solutions exist and both end states are always the same. Even so, it may be possible to rescue such moment approximations. In analogy with the approximation of an irreversible system by a reversible dynamical system of a finite but large number of degrees of freedom, one must take a fixed time (*i.e.*, space) interval and then let the number of degrees of freedom (moments) approach infinity. While this is, of course, a rash extrapolation from the evidence at hand, taking a sufficient number of moments might yield a transfer from a pulse to a decaying periodic wave, followed eventually by a remote regeneration and return to the initial state. In such a case the first half of the solution could be considered an irreversible transition on any physically interesting scale.

## 6. - Zero-temperature solution.

In the limit of zero temperature, the problem is completely solved (at least, for not too large Mach number) [1, 5, 10] (\*). As is shown in [1], the adiabatic two-fluid theory is identical to the exact self-consistent formulation in this limit. One obtains the same periodic and pulse solutions. For large Mach number the problem is not solved, but it is easy to see that no shock (as defined above) can occur. It is important to realize that although this result is exact and exhibits no shock, it is *a priori* evident that the relevant irreversible mechanism is absent at zero temperature, so no shock can be expected.

By a perturbation about zero temperature [11], a pulse solution can be obtained in which the density and magnetic field approach a constant state but other quantities are periodic. This can properly be interpreted as an irreversible transition but it is not yet a shock.

## 7. - Finite temperature, mass-ratio expansion.

By making substantial use of the fact that the electrons are much lighter than the ions, it is possible to approach the full-dress particle equations; see [12], and for a brief account, [1]. To the order of approximation taken, a transi-

(\*) Also: M. H. MITTLEMAN (private communication).

tion is found from the start of a pulse to a periodic wave of smaller amplitude but the ultimate decay is not accessible in this theory. The particle orbits in this theory are exactly what one would expect from the basic dissipative mechanism; the distribution function becomes very distorted after passing through the first wave. The fine structure has approximately the same wavelength as in the elementary adiabatic theory.

### 8. - Weak shock solution.

A recent result (private communication, C. S. GARDNER and G. MORIKAWA) seems to cast doubt on some of the foregoing, or at least invites its reconsideration. In a formal expansion in the neighborhood of  $M = 1$  (small amplitude but not linear), one recovers the pulse and periodic waves as exact solutions to lowest order in this expansion. However, the length scale is larger than the previous one by a factor

$$\sqrt{1 + \frac{1}{16} \frac{m_+}{m_-} \beta},$$

where

$$\beta = \frac{p}{B^2/2\mu}.$$

This agrees with the zero-temperature exact solution since  $\beta = 0$  in that case. However, it implies that the temperature must be *very* low ( $\beta < .001$ ) for this limiting theory to be valid.

The validity of the adiabatic two-fluid theory is now suspect. One expects it to become valid for  $M$  near unity; but it breaks down (for finite  $\beta$ ) in exactly this limit. One can hope for some gross features of the simple theory to be valid for stronger shock and finite  $\beta$ . Occasionally theories seem to be better than their justification or derivation, but this cannot be counted on!

This also creates an apparent discrepancy with the mass-ratio expansion of CATHLEEN MORAWETZ [12]. This is not strictly a contradiction since the latter theory is *a priori* valid only for finite strength shocks (\*).

### 9. - Conclusions.

The significance of even the simplest one-fluid adiabatic theory still has to be spelled out. There is no region of overlap between it and the known solutions of the two-fluid adiabatic theory including the linearized time-de-

(\*) The charge separation field is taken to be large of the order of  $\sqrt{m_+/m_-}$ . More precisely, it has the order  $(M - 1)\sqrt{m_+/m_-}$ . This is large for a fixed strength as the mass ratio increases, but is not large for weak shocks and fixed mass ratio.



pendent piston problem solved by C. S. GARDNER (Section 7 of Ref. [1]; see also [13]). What is needed is the solution of a representative non-linear time-dependent problem to bridge the gaps between the steepening of the one-fluid theory, the spreading of the linearized theory, and the steady non-linear solutions. Recent numerical computations for this non-linear time-dependent two-fluid problem (K. W. MORTON, unpublished) yield approximately periodic wave trains in some circumstances, and, in other circumstances, stable jump discontinuities which develop during the motion. When these results are complete they should greatly clarify the situation.

The correct fine structure of a possible shock could probably be found from an examination of higher order terms in the expansion in powers of the strength, mentioned above in Section 7. Although continuation of this expansion may even lead to a shock, there is reason to disbelieve this since (from the phase mixing concept, as in Landau's damping) the irreversibility may enter non-analytically in the parameter  $(M-1)$ . Even without a strict shock solution, one might in this way reconcile the different length scales in the Gardner and Morawetz theories. Alternatively, it might be possible to refine the latter theory to extend its validity to the realm of weak shocks, but this seems to be quite difficult. On the whole the latter theory is probably the most relevant at this time because of the range of parameters involved.

A continuing study of the stability of the simplest two-fluid solutions would seem to be called for if only to decide on the need for introducing the more difficult and more primitive turbulent-type theories [2].

## REFERENCES

- [1] C. S. GARDNER, H. GOERTZEL, H. GRAD, C. S. MORAWETZ, M. H. ROSE and H. RUBIN: *Proc. Second Internatl. Conf. on the Peaceful Uses of Atomic Energy*, vol. 31, 230 (1958).
- [2] H. PETSCHKE and A. KANTROWITZ: *Bull. Am. Phys. Soc.*, 5, 316 (1960). (Also this proceedings).
- [3] A. A. BLANK and H. GRAD: *Fluid-Dynamical Analogies*, NYO-6486, pt. VII, New York University (July 1958).
- [4] R. COURANT and K. O. FRIEDRICHS: *Supersonic Flow and Shock Waves* (New York, 1948).
- [5] L. DAVIS, R. LÜST and A. SCHLÜTER: *Z. Naturforsch.*, 13a, 916 (1958).
- [6] A. BAÑOS and R. VERNON: University of California, Los Angeles, Dept. of Physics; LRL-no. 1 (1959); also R. VERNON: University of California, Los Angeles, Dept. of Physics LRL-no. 3 (1960).
- [7] M. ROSE: *On Plasma-Magnetic Shocks*, NYO-2883, New York University (March 1960).

- [8] K. HAIN, R. LÜST, A. SCHLÜTER: *Rev. Mod. Phys.*, **32**, 967 (1960).
- [9] J. BURGERS: *Rev. Mod. Phys.*, **32**, 868 (1960).
- [10] J. H. ADLAM and J. E. ALLEN: *Phil. Mag.*, **3**, 448 (1958).
- [11] J. W. DUNGEY: *Phil. Mag.*, **4**, 585 (1959).
- [12] C. MORAWETZ: *Magneto-hydrodynamic Shock Structure Without Collisions*, NYO-2885, New York University (March 1960).
- [13] C. S. GARDNER and G. K. MORIKAWA: *Similarity in the Asymptotic behavior of Collision-free Hydromagnetic Waves and Water Waves*, TID-6184, New York University (May 1960).

## PART IV.

### Considerations on Localized Velocity Fields in Stellar Atmospheres: Prototype — The Solar Atmosphere.

#### D. - Collision-Free Shock-Waves.

#### Discussion.

*Chairman:* L. DAVIS

— R. LÜST:

Let me comment on this one-dimensional treatment and its connection with shock-waves. The first question is why we can treat the plasma as a fluid if we assume no collisions. Normally we apply the fluid picture if you are sure some set of particles move in some way together. This is possible if *e.g.*, the collisions serve as a mechanism to keep the particles together. If there is a magnetic field, and the particles spiral around it, then this provides a mechanism to keep the particles together even in the extreme case of no collisions. But in such a situation, the pressures parallel to, and perpendicular to, the magnetic field differ; so in your equations this difference in pressures must be included. Second, if you want to describe the plasma in this way, you have to be a bit more careful in the terms you retain. Normally, you apply the one-fluid hydromagnetic description, which means the normal hydrodynamic equations plus the  $V \times B$  magnetic term. Also, you apply infinite conductivity, which means zero electric field in a moving system. Especially this last assumption is no more valid in the extreme case we consider. That is, in the conductivity—or more generally call it the diffusion—equation you have

$$(\dots) dj/dt = E + (V \times B)/c + \dots,$$

where you normally set the sum of the first two terms in the right side equal to zero and neglect the other terms. But in the case considered here, when frequencies become comparable to the ion Larmor frequencies, these neglected terms become important. This is the analysis we have applied, including these terms. The 2-fluid description of the NYU group also includes these terms.

So then if you look for solutions which should describe shock-waves, you first find solitary waves. Their wave-lengths are of the order of the geometric mean of the Larmor radii of electron and ion. These are not really shock-waves at all, since all quantities return to their original value after passage of the

wave. To describe a shock, we require some element of irreversibility; and the question is whether we can get it under this picture just described. This question has not been answered yet as far as our treatment goes; I think the NYU group makes the same statement.

It should now be noted that you can get shocks if you are not completely in the collision-free case. In this situation, these solitary waves may be of great interest in creating shocks, because with even a few collisions, these solitary waves get damped and you finally end up with a wave-train where you have the right increase of entropy.

The other possibilities would be to find other ways of increasing the entropy, PETSCHKE has rejected the solitary-wave kind of solution in favor of a set of essentially unstable waves. There are other possibilities of instability; *e.g.*, involving the two streams of electrons and ions. But as a summary-conclusion, it can only be said that we have not yet really obtained a shock-wave in the absence of collisions.

— F. KAHN:

In this problem you are looking for a mean of dissipating energy in a plasma-magnetic wave, and electrostatic instabilities may do that for you. Suppose we have a plasma-magnetic wave propagating perpendicularly to a magnetic field.

The question is: Can the energy of this wave be dissipated without invoking collisions of individual particles. In the regions where the magnetic field is changing rapidly—the regions of increase toward, and decrease from, the wave-crest—the electron and the ion velocities will be considerably different. The ions will not be affected too strongly by the changing magnetic field; and thus you might have a situation analogous to what you get in the ordinary two-stream instability for plasmas, with an electron stream trying to get through an ion plasma. Now, it wouldn't, of course, be right to isolate one region of high current density, and just consider the two-stream plasma instability which might arise there, without information from all the other regions of high density. But I suggest that it would be a relatively easy problem actually to see whether the whole field of such a wave suffers from electrostatic plasma instabilities, because the problem is after all linear. What is more you can readily set up the undisturbed state following the motion of the electrons and of the protons, and then try to disturb it to see if there are any complex frequencies thrown up. That's my first point.

The second point concerns the solitary waves which are known to exist in the presence of a magnetic field. And now I will take an entirely different point of view; supposing in the end it proved impossible for us to construct a collision-free shock, would this be a tremendous disaster? If you consider the problem as it was first put by PETSCHKE, you are essentially asking what



happens when a piston moves into a plasma. Somehow the plasma further ahead has to find out what's going on behind, otherwise you get confusion. Now, if we were doing gas dynamics, we would say that no wave can be propagated fast enough, and therefore we must put some discontinuity into the fluid, which we shall call a shock; in the region of the discontinuity, the equations have to be a little bit modified because we are going to take viscosity into account. But if I'm right in interpreting some of the results on solitary waves in plasma-magnetics, one can get solitary waves of all speeds propagated into a plasma, provided only that one leaves oneself free to consider cases where electrons follow looped trajectories. This case proves rather difficult to consider and I believe in the paper by DAVIS, LÜST, and SCHLÜTER it was not treated. But if such a case is taken into account, I think there is no reason to believe a solitary wave cannot be found which will propagate at any speed you like into the medium ahead. Then, would it not be possible for the piston to find its right place, at any time, in such a solitary wave and to propagate the disturbance ahead in that way?

— H. PETSCHER:

A similar problem to what you describe has been considered by ROSENBLUTH and LONGMEYER: taking a piston, which is impulsively put into motion with a constant velocity, and then calculating the time-dependent equations of motion in a way that is analogous to what LÜST described. I believe that the result that they got was that the density pattern had some wiggles in it, and the thing remained time-dependent. The density did drop off in a distance which was comparable to the thickness of this pulse, which incidentally is about  $1/40$  of the characteristic length that I used, so that if one looks at this grossly it would look like a shock-wave with a dimension of the order of  $V m_e c^2 / 4\pi N e^2$ .

— R. LÜST:

For some astrophysical situations, it is not too essential to get collision-free shocks; but there are situations where it is important. The first time the question has been raised was in connection with accelerating cosmic rays. There, the situation is that the gyro-radius of the cosmic rays is larger than that of the particles having thermal speed in the interstellar medium, but it is smaller than the free path of these thermal particles. If you want to deflect the cosmic rays by shocks in the interstellar medium, you need a shock thickness smaller than the gyro-radius of the cosmic ray particle. So if you have a shock structure which is of the order of the geometric mean of cosmic ray and thermal particle gyro-radii, you have problems; if it were of the order of the mean-free-path of thermal particles, this would be fine.

Then I would only comment that I just did not want to bring in this question of loop trajectories. Neither we, nor the NYU group—with one important exception—have treated them.

— L. DAVIS:

I have a feeling that most of the people who have worked on non-loop trajectories have thought about the loop trajectory problem. You find the equations are much more complicated, but it looks as though the gross character of things would not be very much different if you could manage mathematically to handle the loop trajectories. So, this may be an over optimistic statement, but you start with the simple cases and you hope the others will not be too different.

— A. A. BLANK:

I confine myself to the one question: Is there really any hope for a one-dimensional collision-free shock of the kind indicated by conventional one-fluid analysis? Apparently there is. The work is due essentially to MORAWETZ at NYU and I shall describe it in an intuitive way without writing equations or deriving numbers.

We are seeking irreversible transitions or shocks in a two-particle model. Imagine that we are observing a steady shock accompanied by a magnetic disturbance and that the fluid is passing through the shock from right to left parallel to the  $x$ -axis and perpendicular to the magnetic field.

In general we consider a two-dimensional distribution of particle velocities perpendicular to the magnetic field. The mean velocity is parallel to the  $x$ -axis and we denote its value before the shock by  $u_0$  and after  $u_1$ . At present it is impossible to consider a complete Maxwellian distribution of velocities. Instead Prof. MORAWETZ took a cut-off distribution in the form of a circle centered at  $(u_0, 0)$  in velocity space. Certain particles of sufficiently low velocity can loop, those corresponding to a low velocity sector of the circle in velocity space; the others cannot.

Without going into details of the analysis, let's see how the velocity distribution is altered upon passing through the shock. What we are looking for is some evidence of the Gibbs mechanism for irreversibility, namely phase mixing. After the shock the particles that loop change the shape of the circle grossly. We obtain a picture like Fig. 1*b* in our paper. There is a long ear which follows along the circle closely. You might anticipate that repeated looping will produce further ears, and ears upon ears. This begins to look like the phase mixing necessary to produce a macroscopic entropy jump.

Morawetz's computations yield a transition which shows a rise to a certain height, followed by a periodic wave train at a definitely elevated height. The computations are significant only to a certain distance and the ultimate behavior is unknown.

One feels happy because this result looks like the beginning of the irreversible shock transition we are seeking. One feels unhappy because we can draw no conclusion of what happens far behind the shock. After a certain distance we may return to the initial state, and the irreversible transition be lost. It should be added that in Morawetz's case this seems most unlikely. Once an ear is formed it is hard to see how it could be retracted. Even in this eventuality there is a last hope. Let me remind you of the old story of the Poincaré recurrent time: if we have a box containing a gas and initially 10% of the gas is in one half of the box and 90% in the other, the time will arrive when the gas returns to its initial distribution, but we are not going to wait for it. It is a gross extrapolation in our case, but it is perfectly possible, even if there is a return to the initial state, that the time elapsed may be so large as to be beyond all physical significance. We see, then, that even if it should prove that the transition is reversible in the strict technical sense, there remains the strong possibility that it is for all practical purposes, irreversible.

— L. DAVIS:

While we are still on the subject of solitary waves, let me point out that there is a little difficulty in the terminology here. The words «solitary wave» are now reasonably well recognized as describing a type of electromagnetic wave in a plasma in which the inertial properties of the current-carrying electrons are important, thus giving the wave a remarkably short scale—a remarkably rapid frequency. This is, however, only a more or less singular solution out of all the solutions here. One has a great many kinds of running waves which have the same character of wavelength and frequency and there it not any well-recognized name for these, so they usually get left out in the discussion. But if someone is talking about the motion of an astrophysical medium it may be a very convenient thing to know that one solution of the equations of motion is that of running waves. Now they may be unstable and they may break down after a while—but they do form a useful component in terms of which to describe the whole motions—at least there is some hope they would.

— A. A. BLANK:

We call them periodic wave trains. I have an additional point. There is a result here that does not come out of any other theory. We get differential heating of the electrons and the ions. I do not think there is anything else which gives you this kind of thing.

— F. H. CLAUSER:

BLANK made the statement that Mrs. MORAWETZ worked through the calculations that resulted in the periodic wave train; but did this in fact have the possibility of this phase mixing that you had guessed.

— A. A. BLANK:

Yes, the «ear» did appear.

— F. H. CLAUSER:

Would her calculations have given the additional ear?

— A. A. BLANK:

In a higher order theory, one would expect the next ear to come out one Larmor period later.

— V. D. SHAFRANOV:

I would like to summarize some recent work by R. SAGDEYEV, on some aspects of collisionless shocks in a cold plasma ( $knT \ll H^2/8\pi$ ). He has observed a certain analogy between gravitational surface waves in water of finite depth (following the classical analogy: «shallow water» and plane motion in conventional gas-dynamics). In both cases we have non-linear steady waves of similar structure, a solitary wave, for example. In a magnetic plasma solitary waves exist for  $M < 2$ , ( $M$  — Mach number); for water waves, the critical Mach number is equal to  $\sim 1.7$ . In this region the shock front has a regular oscillatory structure, provided the non-linear wave is stable. Calculation predicts for weak waves ( $M - 1 < 8\pi nTk/H^2$ ) stability with respect to some special disturbances; for example, plasma oscillations. Thus, the small amplitude shock thickness appears to be

$$l \approx l \frac{m_e}{m_i} \sqrt{\frac{H^2}{8\pi nTk}} \ln(M-1)^{-1},$$

where  $l$  is the free path of the electron.

In the opposite limiting case of high  $M$ , an «overturning» of the front appears. The region of multistreaming motion, generated by overturning, does not increase indefinitely. In the case of water waves the force of gravity acts as a turning force. For ions in a plasma the magnetic field plays the same role which turns back the ions in a distance of the order of the Larmor radii.

$$\frac{C_H}{\omega_{H_i}} = \frac{C}{\Omega_0}, \quad \left( C_H = \frac{H}{\sqrt{4\pi n m_i k}}, \quad \omega_{H_i} = \frac{eH}{m_i C}, \quad \Omega_0^2 = \frac{4\pi e^2 n k}{m_i} \right).$$

In both cases the multistreaming motion is unstable. In the hydrodynamical «bore» the cause is instability of the tangential discontinuity. In the plasma this is instability of the interpenetrating ionic streams, moving across



the magnetic field. Such instability develops at a distance of the order

$$\Delta \sim \frac{C}{\omega_0}, \quad \left( \omega_0^2 = \frac{4\pi n e^2 k}{m_e} \right).$$

This leads to an irregular «turbulent» structure of the shock front.

In an intermediate region ( $1 + (8\pi n T k / H^2) \leq M < 2$ ) instabilities of other types may exist, which arise if the regular electronic velocity (in the transverse direction) exceeds the thermal velocity. This corresponds to a characteristic length of order

$$l \sim \frac{C}{\omega_0} \sqrt{\frac{H^2}{8\pi n T k}} \cdot \frac{1}{M - 1}.$$

— R. LÜST:

Let me raise some questions on the one experiment that has been done, by PATRICK. I raise these, not to challenge the experiment, but for better understanding. First, a question on Fig. 1 showing the shock thickness results. If you had just looked at the experimental points, and been unaware of the theoretical curve, would you not possibly have simply drawn a horizontal line, coinciding with the channel width? Second, for the calculation of mean-free-paths, one needs to know the temperature, thus already applies some kind of theory; *e.g.*, the Rankine-Hugoniot equations across the shock. So, how good are the temperature estimates you use? Third, I saw one experimental point, dealing with shock width, lying above the collision shock thickness; what does this mean? Fourth, what is the dependence upon magnetic field direction? This last is important conceptually, since one would expect that for propagation along the field, no collision-free shock should be possible, since only the ordinary free path enters. On the other hand, if I understood PETSCHKE correctly, under his theory one should expect a similar thing to occur for propagation along the field.

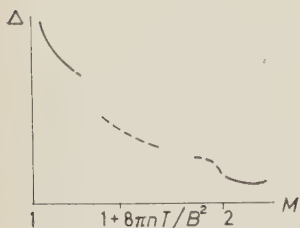


Fig. 1. — The dependence of shock thickness on Mach number.

— H. PETSCHKE:

On question 1, whether the experimental points distinguish between the line drawn and the channel width, I think it obvious they do not. There are two possible effects of the channel width. One is that we are dealing with a cylindrical geometry, and the magnetic field which is driving the shock is stronger on the inside of the annulus than it is on the outside because it drops off as  $1/R$ . This could produce a tilt of the shock which would look like a width

on such oscillograms. We have tried to look for a possible tilt in the shock by looking at different angles. This is rather crude—but seems to indicate that the shock is fairly plane—we are doing further investigations by making a larger radius and same size annulus, to check this point further. The other point is the one raised by LIEPMANN. I think it was that the friction on the walls could reduce the shock thickness. There is not a very clear cut answer. In some cases, we have gotten shocks less than a quarter of the annulus thickness, which makes that somewhat doubtful but it is certainly not beyond being questioned. Now the question about calculating the mean free path. It was pointed out by LÜST that one needs something, possibly the Rankine-Hugoniot equations to calculate the mean-free-path. Now the experimental check of this that we have is that the light intensity measurements tell us the density behind the shock front. Now this slide shows the intensity as a function of initial density: this should go as the square of the initial density, and have a value depending on the density ratio across the shock-wave. Now the upper line corresponds to some rather lower temperature experiments which were done for a shock-wave going along the magnetic field. These were not in the collision-free region—in that case the density ratio should be four and this agrees quite well. In the case of the magnetic field in the plane of the shock-wave, the density ratio across the shock is reduced to 2.2, and we get agreement as far as the density.

This is calculated for  $\gamma = \frac{5}{3}$ ; the density ratio here is rather independent of the  $\gamma$ . You see the main compression across the shock is that of the magnetic field. This is for a shock-wave of about Mach number 2. We tried to see, for example, whether one could tell the difference between  $\gamma = \frac{5}{3}$  for particles and a gamma of  $\frac{4}{3}$  which one would expect if one takes the wave picture seriously, and the density difference was less than 10%. So it is not observable. Now once one has the density, he knows the difference in velocity between the streams ahead and behind the shock. And one can calculate the mean-free-path from this. The next question: there are a set of points where according to the picture, the collision shocks should limit the thickness and the thickness is apparently about a factor of 2 greater than the curve. This is not clearly outside the accuracy of this curve. But it is somewhat of a discrepancy. The last question: PATRICK tells me that one cannot run the device as a whole without some axial-magnetic field. That is, you do not get anything that looks like a shock-wave—it's not clear whether this has something to do with a shock-wave itself; or more probably that it's associated with the pre-ionization mechanisms and uniformity around the ring—some-what extraneous experimental results—so that the minimum angle of the magnetic field to the plane of the shock that has been used is about 15°. He has used stronger axial components of the magnetic field to vary the angle from 15° to 35°. I have not been able to see any dependence on angle; ac-

cording to our theory—one would expect about a factor of 2 between zero and 30 degrees—and this you see is still within the scatter of the experiment so that there is no disagreement.

— R. LÜST:

What about  $90^\circ$ ?

— H. PETSCHKE:

You mean experimentally? One can take the theory that I presented, which has in it the parameters of the mean wave number and the  $\beta$  that one gets, and these change as the angle changes—the  $\beta$  changes because the Rankine-Hugoniot conditions are different when you compress or do not compress the magnetic field. And the wave number changes because waves travel faster along field lines than perpendicular to them. This gives the result that the shock thickness, as the angle increases—goes up by the factor of 2 which was shown and then decreases, and would wind up about a factor of 2 below the zero degree case. The experimental situation on this was that initially the experiments were done with only an axial field as the  $90^\circ$  case; and when one got the condition where the mean-free-path became larger than the annulus, one got no reproducible results. Which is suspicious of the fact that for the  $90^\circ$  case the shock did not exist. However, I think that these experiments should be repeated. So that situation is completely up in the air.

— H. LIEPMANN:

I agree with these points but I have two more. One is that the shock has to do the ionizing — in front you have no conductivity — so you have to come up through the shock to the conductivity you need, and this looks to me somewhat difficult to interpret theoretically. I realize very well, of course, how difficult the experiments are. The appearance of a shock with the axial-magnetic field could be interpreted differently. It looks to me that with an ordinary shock-wave, with pressures and diameters that you have you don't get a shock, not so much because of friction at the wall but because of heat conduction to the wall. Because nearly the whole mass of the gas is boundary layer; the wall is cool, the density high, and consequently the whole mass of the gas is there and what happens is that you have a negative displacement thickness. So you don't form a shock. So it is possible that the axial-magnetic field mainly produces the heat transfer to the wall. If this is the case you will find no shock for zero axial-magnetic fields. This is, I think, a very strong effect on the possibility of even forming a shock, collision-free or otherwise.

— H. PETSCHKE:

Yes, the argument for always having some axial field is that this tends to keep the gas off the walls. Now the type of thing you suggest — of the gas

cooling the walls. — would lead to a measured density which was off from the one you would expect. So the agreement of the density curves to some extent supports the idea that the walls are not particularly important. As far as the question of the ionization of the gas — that is a shock-wave going into hydrogen at room temperature conditions — the temperature that you would get behind the shock at these shock velocities corresponds to a million degrees. Now at something like 100 thousand degrees, the gas should already be well-ionized. If one calculates the rate of ionization, this depends on electron collisions.

— H. LIEPMANN:

Not initially?

— H. PETSCHKE:

Initially it is not clear — if one has some electrons, the time in which the electrons would double themselves is still an order of magnitude smaller than the shock thickness we are discussing. So, presumably, what is happening is that in the first part of the shock, until one gets to say 100 000 degrees — there are still collisional effects important and ionization is going on, but this is only the first 10 percent of the shock-wave.

— L. BIERMANN:

Do I understand correctly that one should expect some non thermal electromagnetic radiation from the fluctuations of these wave packets which move about in your picture. If so, did you develop the theory of the emission in the frequency range of some multiple of the gyration frequency of the ions or neighboring frequency ranges? And did you make any attempt to discover experimentally whether excess radiation exists — that might be a good means, if the theoretical expression can be derived. There might be a possibility to discover experimentally whether you actually get the fluctuations as a sort of turbulence or not.

— H. PETSCHKE:

The large amplitude waves which are present in the plasma around the ion cyclotron frequency should give rise to some radiation which could be observed outside. It is somewhat difficult to estimate how much, because as these waves hit the boundary from a very sharp boundary they would be reflected. If the boundary is more gradual, it is not clear how much is reflected and how much is transmitted. We are in the process of trying to measure field fluctuations just outside the plasma with a pickup coil. These experiments have not produced anything yet. Another experiment which is in pro-



gress is to try and find whether the waves do exist by shooting an electron beam which goes through a curved path and comes back. The position where it comes back tells you the strength of the magnetic field — the average magnetic field which is an interesting quantity. The de-focusing of the beam should tell you the fluctuations that are present. This experiment has just barely gotten started; there are no data yet that I know of.

— M. KROOK:

We have been confronted with two kinds of treatment of the collisionless shock this morning, and told there were allegedly only two schools of thought. There are at least three: Liepmann remarked in an aside earlier that he did not believe any of it. So if these are three views, there is a fourth. My own theory is that the way to discuss the collisionless shock is not by invoking the Boltzmann-Vlasov equation but actually to reexamine the problem anew and to find new equations of motion. Once we abandon the collisions in the Boltzmann equation, then we have to find a dissipative mechanism to take its place. Let me sketch an approach.

In the ordinary treatments one has a set of distribution functions for each kind of particles, one-particle distribution functions. We have an electric field  $E$  and other fields as well. We write down equations of motion for the distribution function. This is the Boltzmann-Vlasov equation in which the  $E$  is determined through an equation involving the charge density. Now then we throw away the Boltzmann collision term, which is itself an idealized representation of molecular interactions, — that is that particles interact only when they get close together. Once we have thrown that away we have lost the major dissipative mechanism, and have to re-examine the equations of motion, to actually remove another idealization — which is inherent in those equations of motion. Now one way of doing this is to replace this  $E$  by an  $E_0 + E_1$  and to say that  $E_0$  is determined by the Poisson equation, and the  $E_1$  can only be specified stochastically, because there are microscopic fluctuations which are averaged when we write down the Boltzmann equation. When you do this, this fluctuating field  $E_1$  now gives you a dissipative mechanism. Actually Thompson has discussed conductivity from this point of view by putting in this field, and Florence at Harvard has considered the scattering of plasma oscillations by these fluctuations  $E_1$ .

— H. PETSCHER:

You say one should emphasize the long range forces due to charge accumulation. Now this type of thing leads to the wave motions, and this is precisely the term that we have emphasized.

— M. KROOK:

What I mean here is that the  $E$  which you put into the Boltzmann equation is a macroscopic  $E$ . It is a macroscopic field — a self-field — due to charge accumulation and so on. The  $E_1$  is a fluctuation in the two-particle correlation function. It is not enough to just write down the two-particle correlation function itself, but you have in fact to work out what the fluctuations are in the field that a particle sees, due to screening and so on. The characteristic length for this  $E_1$  is of the order of a Debye's length — where there is no magnetic field, and the characteristic time is something of the order of the plasma frequency. For example, if you work out the scattering of plasma oscillations, with  $E_1$  absent — in other words just with the  $E_0$  — and then put  $E_1$  in as a perturbation — then the quantities that enter are components of  $E_1$  at  $x$  and  $t$ , and  $E_1(x)$  at  $x'$  and  $t'$ . The average values of the correlation function are involved. These are microscopic fluctuations as opposed to the character of  $E_0$ , which is a macroscopic field. The plasma oscillation is an organized collective motion of the medium as a whole,  $E_1$  is not a collective motion — it is collective only at most within the Debye's screening radius — or rather a radius of that order.

— R. LÜST:

I completely agree that one has really to investigate what are the proper equations for the plasma if the collisions are not there, therefore one has to investigate fluctuations and their influence.

From the other side I think the situation might be somewhat different if you have a strong magnetic field. And this is our attitude, for instance, when we say that we still have a fluid description. Then if we take the Vlasov equation, we have to add the magnetic field terms, and our approach is that these are the largest terms and may replace the collision terms.

— M. KROOK:

I think my criticism is based on a much more fundamental starting point than the one mentioned here, if I may say something about that. This is actually involved in the definition of the distribution function. There are two possible ways in which one can define such a distribution function. There is the classical way, where one tries to define this as a function of  $r$ ,  $x$ , and  $t$ . To define such a distribution function, you take a small volume, and if you read CHAPMANN-COWLING, you are told that this volume must be sufficiently large to contain a large enough number of particles so as to smooth out fluctuations, and at the same time so small that you can effectively regard it as infinitesimal. Now, of course, mathematically this is nonsense; and what one would have to do is to write down say difference equations, and after a time you would not know from which particular cell a particular particle came. All you can say about it in

this treatment is that it was in that particular cell, and at a later time it could be in one of a number of different cells. One way out of this apparently is to go to an ensemble, and to say that in fact we consider a large number of identical systems — prepared in exactly the same way as the system of interest, and then we can define this by taking an average over the ensemble. But when we write down the equations of motion for these distribution functions, they are not then the equations of motion of any real system, because the forces to which particles are subjected are forces averaged over the ensemble. In other words fluctuations have been smoothed out, and your equations of motion are not the equations of motion of a real system at all. If you want to take account of fluctuations, you have to put in some new physical assumption. In the Boltzmann case it is the molecular chaos assumption, where you in effect say that once a particle has been in collision you do not follow it out, but next time it collides you specify only that the impact parameter has a certain probability distribution. Once you throw away collisions you must take out some other idealization, which is inherent in this kind of a definition of a distribution function, and the writing down of an equation of motion for it. One way of doing it is to allow for the fluctuations not only of the electric field but of all physical quantities involved. And I think this would also operate in the case of the presence of a strong magnetic field. It may not be important in those cases—I would not go so far as to say that it is always the dominant term. You may be right, that if there is a strong magnetic field—then this fluctuating term has a minor influence as compared to others.

— H. LIEPMANN:

I am wondering whether your ideas are related to some recent work of GREEN (H. S. GREEN: *Phys. of Fluids*, 2, 341 (1959)), who derives macroscopic equations for a conducting gas. GREEN rejects the usual phenomenological introduction of Ohm's law and instead discusses statistically the collective influence of all other particles upon the forces exerted on a particular one. In this fashion the effective dielectric parameter and the conductivity can be expressed in terms of correlation functions.

— M. KROOK:

What is the dielectric constant?

— H. LIEPMANN:

That is the question which GREEN discussed, isn't that right?

— M. KROOK:

Yes, I think so, but not quite in this context.

— W. V. R. MALKUS:

In most of these studies, particularly the laboratory studies, one is talking about situations with magnetic fields initially imposed upon the system. Then one explores plasma instabilities, and the more microscopic instabilities which result from the existence of the field. Certainly one good reason this is done is that these instability problems are then linear. Now there also exists the possibility, particularly in this shock column we have been talking about, that we have a finite amplitude instability which involves the production not only of fluctuating velocity fields, but fluctuating magnetic fields. This is a generation problem in which both fields arise simultaneously from the available potential energy, or in this case the available organized kinetic energy, which is then turned into disordered kinetic and magnetic energy. Now the imposition of a magnetic field to rationalize the degeneration or the production of the shock leaves one of course the problem of where the magnetic field came from. Many of the fields that are being discussed astrophysically are of such scale that they cannot have existed primordially, and therefore they must be produced by some local kinetic process. I suggest that the two are the same; that the instabilities that are being explored, and the magnetic fields that are in this argument used to generate them, are both produced by the same instability. Now one can have some faint test of this. We have been listening to detailed mechanistic inquiries into possibilities—perhaps a less mechanistic approach would be to inquire into extremes that the instability could produce in terms of absorbing energy from the available organized flow and putting it into a disordered flow. In some work in the *Astrophysical Journal* last summer, I explored another explicit mechanism for the production of—let's call them extreme magnetic fields—and in that case and perhaps in this case too—if one wants to absorb as much energy as possible from the streaming organized flow, put it into disordered flow, one strikes a balance between the advection of momentum  $V \cdot \nabla V$  and the advection of momentum by the magnetic flow  $(\mu/4\pi\varrho)(H \cdot \nabla H)$ . This balance permits the greatest release of the available organized energy into the disorganized form which arises from the instability. Now an estimate of whether this is indeed the case, can perhaps be made by comparing the magnitude of these quantities in this shock—or the shock one might expect if indeed such a quasi-mechanistic equipartition occurred. In interstellar space I recall we are discussing a region where densities of  $10^{-21}$  and velocities of  $3 \cdot 10^7$  cm/s prevail. By comparing the magnitude of the fluctuations that must exist across the shock—one can get some estimate of whether the magnetic fields that result are in keeping with those one anticipates there. Such a balance is also an equipartition of magnetic and kinetic energy. We have then

$$\frac{1}{2} \varrho V^2 = \frac{\mu}{8\pi} H^2,$$



where  $V$ , the fluctuating velocity, is  $\frac{1}{2}$  times the organized flow velocity, the temperature behind the high speed streaming being very small. This suggests that  $H$  would be of the order of  $10^{-4}$  gauss in the vicinity of the shock. I do not know if that is an unreasonable astrophysical number—the mean numbers in the interstellar region are smaller than this from the other observational data; but perhaps just in the region where the field is being produced, one might anticipate it being larger, diffusion mechanisms perhaps would make it smaller over a broader scale. Now, I might note that if the mechanisms proposed—of Alfvén waves being the principle motions that exist as the macroscopic, randomizing process in the shockfront—are justified, they also have the property of an equipartition between their velocity and their magnetic field. Therefore this argument is not incompatible with the thought that the principal physical quantities involved in this a-mechanistic equipartition momentum balance are Alfvén waves,

$$V_A \simeq \left( \frac{\mu}{4\pi\rho} \right)^{\frac{1}{2}} H_A.$$

Is that right; is, in an Alfvén wave, energy equipartitioned between the fluctuating velocity and fluctuating magnetic field?

— H. PETSCHKE:

For an Alfvén wave at frequencies below the ion cyclotron frequencies—that is true. However, the wave would be important at frequencies somewhat above the cyclotron frequency, and in that case the magnetic energy in the wave is larger by a factor  $\omega$  divided by the ion cyclotron frequency.

— W. V. R. MALKUS:

That being so, this would be the minimum value for the magnetic field; so that in fact you expect larger magnetic fields associated with the fluctuations. One last point I might make is that I do not think the kinetic theory viewpoint is incompatible at all with a continuous mechanism. We cannot make any plausible arguments that can be tested about the independence of one unstable wave in its initial period of growth from all others, but the idea that there is available energy for the growth of waves and they will grow from the white background noise in disordered fashion and lead to a disordering of the available kinetic or potential energy is far from implausible. I wanted to note that it is the basis of the theory of turbulence that I presented to you earlier in this conference.

— L. DAVIS:

This was an interesting discussion of essentially the dynamo theory of the generation of magnetic fields, by motions of matter. This is an important thing because clearly the origin of all these magnetic fields that we talk about

is highly important. However, it is somewhat outside the field of our present discussion. It has been discussed and a good deal has been published on it in earlier times, so I suggest that we discuss the other things that seem more directly concerned with stellar atmospheres. I would only remark that for the interstellar space, which we are now discussing, the velocity of  $5 \cdot 10^7$  cm/s is at least one and probably two orders of magnitude higher than most people would allow, except in exceedingly trivial size regions. This velocity is nearer the values discussed for the solar wind.

— N. MILFORD:

I wish to comment a little on the picture of the shock region between the solar wind and the interstellar gas. We have the usual picture of the solar wind, with a density of 100 particles  $\text{cm}^{-3}$  and velocity of 500 km/s at one a.u. It was suggested that somewhere in the region of 100 to 1000 a.u. we had the beginning of the shock front. The density there is down to  $10^{-3}$  and the velocity is supposed to be effectively unchanged. Then in the shock region beyond this distance, presumably, the density is of about the same order and the temperature is very high; and then out a bit further we have the interstellar gas with densities of the order of 0.1, velocities of the order of 10 km/s, low temperatures, and mostly neutral.

Now if this shock thickness is to be anything like one collision-free path—let's discuss the situation without magnetic fields and no other stars present—then with these densities and temperatures you get a distance of the order of  $10^5$  or  $10^6$  a.u. If we have a relative speed of 10 km/s between the interstellar gas and the sun, then it seems to me that the interstellar gas will tend to penetrate much further into the system than is indicated by the appearance of this shock, and that it will come into a distance from the sun of the order of some few a.u. with the sort of figures we listed before. The actual result depends, of course, upon the collision cross-section for charge transfer and can be put into the approximate form,  $R_{\text{a.u.}} \sim 16[(1 + 10^{-16}/\sigma)^2 - 1]^{-1}$ , where  $\sigma$  is the cross-section for charge transfer; if you put  $\sigma \sim 10^{-16} \text{ cm}^2$  this gives us about 5 a.u. So at this distance the interstellar gas becomes substantially ionized, then it is stopped by the solar wind in a short distance, and finally swept out with the solar wind. The picture that we have then for the density in the interplanetary medium near the solar system is something as follows: the inner part is the same as previously postulated, but a little further out, say at about 5 a.u., you have the solar wind interacting with the interstellar gas. The interstellar gas probably tends to pile up to some extent in this region and may have a density ten times larger than its original value. Further out we have a region in which the solar wind density appears to be less than the interstellar gas density, before we come to this shock front which was mentioned last week.

Now this is without magnetic fields. If we put in magnetic fields presumably the region is compressed somewhat towards the sun; but unless there is a tremendous increase in density somewhere, the interstellar gas will still penetrate a considerable distance into the solar system with this value of  $10^{-16} \text{ cm}^2$  for the charge transfer cross-section. So the questions that I would like to ask the aerodynamicists are as follows: 1) What would they call such a region of interaction if they believe it exists? 2) Under the conditions we have talked about, in the presence of magnetic fields, how far do they think the shock front would be liable to extend, particularly in view of the variable nature of the interstellar gas further out (variable density and probably velocity fluctuations also)? Finally, if there were large fluctuations in the solar wind, for example during times of solar disturbance, and there were a shock front as suggested, what would happen if a very large flux of particles came along? Presumably it would punch some type of hole in the shock front and region out to some distance, but what would happen after that—would there be then some sort of oscillation of this protruding front, or would it just die away gradually?

— E. N. PARKER:

The galactic magnetic field will in fact stop the solar wind, at say 500 or 1000 a.u. if the present figures are correct. I do not believe the  $10^5$  a.u. model.

— M. J. SEATON:

I would like to raise the question of the temperature of the solar corona, particularly from the standpoint of the energy distribution that one would expect in a medium heated by shock-waves. I think it is worth summarizing the position and suggesting what the possible explanations might be.

It has been mentioned once or twice before that one can get the temperatures of the corona from the ionization equilibrium; the temperature usually quoted is  $1 \cdot 10^6$ . One can also get a temperature from line widths; this is usually given as  $2 \cdot 10^6$ . I think there is a general impression that this is a discrepancy which will be cleared up, but in fact it seems now fairly certain that this discrepancy is real. Let me give a little more detail. The ionization that is of interest is for ions Fe X to Fe XIV. These ionization equilibria depend only on the energy of the electrons and one could say this gives a temperature of the electrons. It has been thought that the most uncertain quantity entering is the collision cross-section. Recent work by BURGESS in London has been concerned with a systematic study of cross-sections for highly ionized atoms. This work indicates that one cannot in fact change the adopted cross-sections very much. Similar work has recently been done in Munich by Miss TREFFTZ. With these latest results one has a figure rather less than  $1 \cdot 10^6$ , although not very much less. We then have  $T_e < 1 \cdot 10^6$  for the electrons. In

order to get up to  $2 \cdot 10^6$ , the cross-sections would have to be in error by a factor of 30 and this seems extremely improbable. On the other hand, consider the line-width measurements. This is a very direct measurement, and the latest results give a value rather bigger than  $2 \cdot 10^6$ , more like  $2.5 \cdot 10^6$ . It would seem that the measurement is sufficiently definite there that one could say that  $T_A \geq 2 \cdot 10^6$ , where  $T_A$  refers to the temperature of the emitting atoms.

If this is a real discrepancy, consider what might be the possible explanations. One would be that the electrons do not have the same temperature as the atoms. This depends on relative magnitudes of various relaxation times, and the general opinion seems to be that one would have the correct numbers only in the outer corona—although I have not checked the numbers myself. A second possibility might be that there are non-thermal velocities; in that case this would be important for the atom, but the corresponding velocities for the electrons would not be important. If one took these as turbulent velocities, he would have the turbulent velocity of the same order of magnitude as the thermal velocity of the heavy particles. The third possibility might be that there is no temperature at all—that is to say that we do not have Maxwellian distributions. And in this case one notes that, while the ionization does not depend on the extreme tail of the distribution, it does depend on the distribution of rather high-energy particles—rather higher than the average. The line width will, of course, depend on something like the mean velocity. I would like to have comments about the sort of velocity distribution people might expect for the electrons and atoms for a medium heated in the way that the corona might be heated.

— E. N. PARKER:

The only comment I can make on the possibility of a non-Maxwellian velocity distribution is that once a long time ago we applied the order-of-magnitude numbers appropriate to the corona to some general models for Fermi acceleration of particles, and concluded that while we could rather easily make the ion velocity non-Maxwellian, we saw no possibility of making the electron velocity distribution very non-Maxwellian. I am not saying that the possibility does not exist but it is not obvious how electrons could be accelerated on the basis of the kinds of things considered so far for particle acceleration.

— C. DE JAGER:

Could not the non-Maxwellian shape of the velocity distribution function arise from the fact that the high-energy tail of the distribution is continuously lost to space?

— E. N. PARKER (and others):

Discussion on direction of the effect. Agreed this is a possibility—but must be computed.



— N. PETSCHER:

If one assumes that all the heating goes initially into the ions, did you say that one *cannot* explain this difference?

— M. J. SEATON:

This is the sort of possibility that has recently been considered by ALLEN. Certainly most of the radiative cooling might be by the electrons, and one must consider the possibility of the heating going in the first place to the atoms. But this is a matter of just computing relaxation times for these processes; such calculations made some years ago suggest the in-balance only occurs at very low densities, thus in the outer corona.

— N. MILFORD:

Perhaps in Kahn's absence I can quote his dinner table estimates of relaxation times in the lower part of the corona. I believe that he estimated relaxation times of the order of some few minutes.

— R. N. THOMAS:

KROOK should correct me if I make a misstatement here. But we calculated this difference between electron temperature and atom kinetic temperature some time ago. We did it for somewhat higher densities, and neglected the effect of impurity cooling; that is, we considered only hydrogen, not things like the metals, or carbon, oxygen, and nitrogen. We got essentially no difference in temperature—something like  $\Delta T = 1000$  degrees at  $10^7$  °K. But if one puts in some impurities, then our results become completely uncertain.

I think this last is certainly something that can be done once we know what the energy is that can be put out by these impurities. This last is the unknown part—term diagrams,  $f$ -values, collision cross-sections; not the methodology of making the calculations. So, our investigations should be re-done for coronal conditions.

## PART V.

### Summaries.

*Chairman:* F. H. CLAUSER

— H. LIEPMANN:

I have been asked to give a short survey summing up the general impressions of the meeting by an aerodynamicist—or better, by a non-astrophysicist. Since my remarks have to be made with little time for preparation and on the basis of limited notes from the meeting, my presentation is quite subjective and I have to apologize for my omission of some important points.

First of all in comparing the present Symposium with the previous ones in 1949, 1953 and 1957 I am impressed with the very much improved balance in the topics and interpretation at the present Symposium. The three previous meetings were each concerned almost entirely with a single group of fluid mechanics phenomena: Turbulence, shock-waves and magnetohydrodynamics in this order. In the present Symposium all three of these seem to have found their place and the preoccupation with a single one of them has passed. In former Symposia I felt often completely frustrated by my inability to distinguish between an observational result and a highly opinionated interpretation. Frequently an astrophysical problem which sounded quite interesting for a fluid mechanician was presented, only to be immediately torn to pieces in a discussion or in a subsequent talk. In the present symposium I have not felt this way at all and I do believe that the preparation of survey talks helped very much toward the presentation of a more balanced and stable astrophysical picture. I hope that the astrophysicists feel similarly about the survey given by non-astrophysicists.

A few quite fascinating problems for fluid mechanics have been brought out in this conference. First of all the problem of cepheids was excellently introduced in Ledoux's lecture and it looks to me that the problem of pulsating stars is a rather well-defined and challenging problem. Observed is quite a stable and very exactly periodic oscillation of a gaseous system. Off hand there are two possible models which show such a behavior.

One may think of a linear oscillator with slight positive damping excited by random forces. A typical case of this type is the Brownian motion of a torsional balance which exhibits an oscillation in its natural frequency with

a constant mean amplitude with more or less pronounced beats. The second model is a non-linear oscillator for which positive and negative damping cancels at a certain amplitude and which thus oscillates with constant amplitude in a limit cycle.

This latter model seems by far the more likely in view of the observations which show apparently a very constant amplitude and no beats whatsoever. This problem look to me very promising indeed and I feel that much progress can be made in the next few years. I understand that CARRIER is already actively working on such a model. I am sorry indeed that SEDOV was unable to join us here since he has been working with models of pulsating stars for some time with more emphasis on the actual motion in the stars. The oscillator model should serve to clear up the mode of energy transfer to the oscillation and it should then be possible to connect the two approaches to give a reasonably complete theory of cepheids.

The second well-defined fluid mechanical problem discussed at this Symposium was brought up in DEUTSCH's paper in connection with the efflux of matter from stars, in particular the solar wind. Reduced to the bare essentials we deal here with the spherical symmetrical source-like flow of a compressible fluid in a central gravitational field. The streamlines of source flow are everywhere divergent. The gravitational field has an effect upon the motion which is equivalent to the appearance of a throat in the streamline pattern of a force-free fluid. Hence the motion can be reduced to the well-known problem of flow in a convergent-divergent channel. CLAUSER and GERMAIN have discussed this problem at length and GERMAIN has presented it in a very elegant and simple form with which one can easily discuss all possible flows of this general type.

The model used is obviously over-simplified: Neglected are rotation, magnetic field effects, and the fluid is considered non-viscous. Thus shock-waves appear as sharp discontinuities; and this is a suitable idealization only if the shock thickness is small compared to its radius of curvature. In any case the model can serve as the basis for more refined calculations and for an overall representation of the simplest possible steady flow from a star.

The fluid mechanics of the convectational heat transfer—the convection zone—I find still a fascinating subject. I am very sorry for having worried so many people with my derogatory remarks about mixing-length theory, being unable at the time to supply any substitute. What I mean was mainly to say that a theory which was not really well-founded should be treated with caution. I also cautioned about the application of the Kolmogoroff turbulence theory to gases with high random velocities. Now, on the convection problems: first of all I feel that there is still quite a bit of possibility of experimentation. From the viewpoint of purely fluid mechanics, I do not consider the problem of convection and turbulent convection to be solved. In the work of MALKUS,

there are certainly some good and possibly even deep ideas; but I am not prepared at this time to analyse it completely, simply because I am still not able to understand it fully. But here we have a problem that is really difficult, and I believe that one cannot estimate the years necessary for its complete solution. In any case it could stand quite a bit of experimental and theoretical work.

I would like to add one point which I don't believe was stressed sufficiently here: In general, a sharp division was made between laminar instability and convection cells and turbulent flows. There are other cases of turbulent motion which are similar to convection zones, notably the flow between rotating cylinders. Here the centrifugal force takes the place of the buoyancy, and the instability here appears in the form of the Taylor vortices which are the equivalent of the Benard cells in convection. In flow between rotating cylinders it is known from the experiments of MACPHAIL, PAI and COLES that it is also possible to have a *turbulent* basic flow and superimposed on that, cellular structure. So, that an instability motion with a definite pattern is superimposed on a fluid which is already turbulent, and behaves like a fluid with different transport parameters. A similar motion has been observed very recently in the vortex shedding in the wake of a cylinder. Here ROSHKO found vortex shedding on top of a turbulent wake. I feel certain that something like that is possible with convection cells. Hence there is a good possibility that the granular structure as observed by SCHWARZSCHILD, and by RÖSCH at the Pic du Midi, are not quite regular, not quite as you think Benard cells should look, but not as irregular as the intermittent boundary between a turbulent layer and the outer flow discussed by CLAUSER. The Benard cells are superimposed on a turbulent medium quite in line with Goldstein's remarks, that the observations show a superimposed pulsating motion. So, it is not simply the problem of distinguishing between laminar instability or turbulent motion and turbulent instability. A particular laminar flow can be unstable to very definite types of structure—to waves, to vortices, to cells. These superimposed structures can grow, and may then exist in a stable form in certain cases. Eventually the flow may become turbulent, and the turbulent flow may also exhibit a large-scale structure, which we sometimes used to call the superstructure. The flow made up of cells superimposed on a turbulent motion will have a continuous power spectrum as well but with a clearly different low frequency component. The scale of the superstructure differs from the scale of the turbulence in much the same way as the scales of the laminar instability modes differ from the mean-free-path. This whole field of, say, «turbulent instability» can use much work.

Connected with this problem there is an equally interesting and to a certain extent not quite as difficult a problem, the excitation of the sound and shock-wave field which is supposed to heat the corona. Actually, of course, one



should solve the whole problem of heat transfer in the sun in one step: The actual velocity field can be represented by a superposition of a sound field, a convective cell field and a turbulent field, possibly a field of hydromagnetic waves has to be added as well, and this *one* model should lead to the temperature distribution including the corona. Such an approach is probably too difficult, so one makes a «step» at the convection zone and only from there on discusses the sound field. I would like here to emphasize a remark made earlier by CLAUSER, about the difference between sound waves and turbulence, in the terminology which we are used to. You can do this in several ways: Mathematically a velocity field can be made up of a field which has zero divergence and finite curl, and one which has zero curl and finite divergence. The former you call turbulence; and the latter, sound. This is not quite complete, because you can also have temperature spottiness and hence an entropy field. Turbulence exists, as you all know, in an incompressible fluid. The equations of an incompressible fluid are elliptic, or, in certain cases, parabolic, which means that if you disturb it at one point, you produce not a wave but a diffusion pattern. Sound is governed by hyperbolic equations, and if you disturb one point, waves propagate outward. Consequently, a random sound field is really a stochastic field of waves in the strict sense of the word, but in a turbulent field, we deal with the stochastic field of diffusing elements. I am not sufficiently familiar with the observations on the sun. Observations of a random sound field and a random turbulent field in the laboratory differ in a characteristic way which can be observed in photography of a fast-moving shell. Here one sees the boundary layer and also a sound field (cf. Fig. 1 in Part IV-A: discussion). There is a characteristic difference in appearance. In the boundary layer, the structure looks more spotlike and round and the sound field looks more like entangled spaghetti. And this is, of course, typical of the two cases: hyperbolic equations tend to give you fronts—mixed up fronts of sound waves; while parabolic or elliptic regions give you more or less circular structures around the point of diffusion. The point of disturbance will diffuse out. It is not always as clear as that. In these pictures of the sound radiation from the boundary layer of a fast-moving bullet, it is obvious. You can distinguish the two fields exactly. And it is clear then that diffusion fronts will exist even if the motion is incompressible: *i.e.*, in the limit of the incompressible approximation, the sound field will vanish but the diffusion field will remain. I think I will take a chance here of boring the audience with a model I have used very often, to illustrate the coupling between turbulence and sound. I had a great deal of conceptional trouble at the time I got interested in the problem and I found one model which helped me: First of all, note that the pressure in an incompressible fluid is nothing but a constraint; the pressure  $p$  is introduced to keep the condition  $\nabla \cdot V = 0$  satisfied,  $p$  enters as a Lagrangian multiplier in deriving the

equation of motion to take care of the auxiliary condition of zero compressibility, exactly like any constraint in mechanics. In terms of the motion of an ordinary pendulum—and this is the analogy I am going to draw—the force exerted by an inextensible string, the constraint which keeps the pendulum on a spherical surface, is exactly equivalent to the pressure in incompressible motion. Consider the motion of this pendulum in the analogy as the incompressible turbulence. If one admits compressibility, one relaxes the condition that the string is inextensible. As the pendulum moves back and forth it sets up oscillations in the string; the coupling between the two is exactly like the coupling between turbulence and sound-waves. Namely, for small Mach numbers—for small energies in the turbulent motion—you can see immediately how you would compute this: compute first the pendulum motion, taking the string inextensible and the fluctuating force, acting on the string. The oscillations set up in the string by these forces are the analogue to the acoustic radiation. This is the exact equivalent of the Lighthill theory. He computes the pressure fluctuations neglecting compressibility effects and then applies these to a compressible medium. It is evident that such a procedure must break down if the energies in these two motions become of the same order. In this case the turbulence and the sound have to be treated as a strongly coupled system. This already complicates the pendulum problem; to discuss a three dimensional continuum on this basis is really quite unpleasant. In any case if the energies in the turbulent motion become large, the Lighthill theory must cease to be correct. *E.g.* the 8th power law ceases to be valid—indeed it must or at  $M = 3$  no turbulence would be left anymore. In any fluid with high energy random motion, a mixture of these two modes—sound and turbulence—must be found; but in different ratios depending upon the energy of the motion. On the other hand, from observations of, say, the solar atmosphere it may be difficult to tell the difference. One will not be able, as far as I can see, simply from observing the Doppler distribution in a given direction to distinguish between what is sound and what is turbulence, from one observation at least. In the laboratory it can be done by checking on the phase relation between the fluctuation in velocity and the fluctuation in density, or between the fluctuation in density and fluctuation in temperature. Since sound is essentially an isentropic process, even when it becomes pretty strong, a very definite phase relation between pressure and density exists and therefore between pressure and velocity and therefore between density and velocity and so on. Hence this phase relation has to be used to discriminate the modes. Consequently if Doppler observations yield velocities which are of the order of, or more than, the velocity of sound, the existence of hypersonic or supersonic turbulence does not follow, but it may mean the existence of a random array of sound and shock-waves.

One other feature of the solar observation was not—I believe—adequately

discussed: The very sharp demarkation zone between the umbra and the penumbra in a sunspot. If one looks at fluid mechanics in general, the chance of having a sharp transition zone exists in a few cases only; one is a shock-wave, and the other is a vortex sheet, which can be quite sharp. Now it is true, as PETSCHER pointed out, that radiation may exaggerate the sharpness of such transitions. *I.e.* an exponential dependence on the temperature may make a smooth but steep transition look very sharp. But still, there is the problem that I think one should come up with a reasonable idea why does the umbra-penumbra boundary look so sharp. And from the pictures I have seen, it is really quite surprisingly sharp. In general, I have the feeling that while it may be too early to get a complete model of the sunspot, from the observations it may be possible to at least draw a kind of kinematic map which combines the magnetic field, the velocity field and the density field in such a way that one is left without any real strong contradictions in the magnetohydrodynamic behavior of the fluid. And I think we are almost up to that point; I think the observations are such that within the next year or so one should really be able at least to draw something like a reasonably complete streamline and magnetic line diagram as a basis for more refined models.

Similar thoughts came to me during the discussion of flares: Here too I think that one may be able to take a set of observations and try to form an overall aerodynamic model not so detailed that one is capable of describing the mechanics of the phenomena, but at least to tie different features together in an overall way, free of obvious contradictions. I feel that here progress will be made; *e.g.*, in the symposium we discussed whether the photosphere below a flare is undisturbed. From the contributions here, mainly by PARKER, we came to the conclusion that we just aren't sure. No large disturbance is observed but it is possible that the disturbance dies out too fast, so that when the flare is past the surface is already quiet. But it may be possible to add here to the observational facts.

The whole problem of magnetohydrodynamics of rapidly changing and slowly changing phenomena on the sun is one I think fascinating for anyone working in fluid mechanics. Needed here from the astronomers is a short list of observational facts. We have gotten part of these already here; namely, a sort of map on which the observations, devoid as much as possible from any speculations, are put down and the implications for other phenomena are only indicated. CARRIER made a strong point concerning such information and suggested that the astronomers give him a sort of «zero-order-time-table» in which it is plainly stated what is observed, what is deduced, and what is the limiting error, and no speculation. This time table is to serve as a sort of reference for the aerodynamicist to start thinking, and then go to an astrophysicist to argue.

Now lastly I would like to add a few words on the problem of collision-free



shocks. We can leave aside for the moment the question whether such shocks are really needed to describe the observations. In any case there is no doubt that we have to be able to understand them. We have to be able to understand whether collision-free shocks exist; and if so, what is their general structure? Now it looks to me that this is a problem that should be solved within the next two years or so. The models, as you have seen, are still somewhat contradictory. None of the models have yet led to a solution in the sense that you can present it with your hands tied behind your back. And they still involve a lot of argumentation and loose ends. Especially since terrestrial experiment here are very difficult. There is really only one set of experiments—Patrick's experiments—and I have the feeling that the theories and the experiments do not agree, but they lean on each other, and that if you pull one out, the other falls. This is not exactly the way a theory and experiment eventually should behave. But there is no doubt that this problem leads into some really fascinating fundamental questions about the general nature of the plasma equations. Also one is forced to consider boundary conditions, which plasma physicists from my experience do very reluctantly. The general equations are usually discussed in an infinite domain. In the actual case one is forced to deal with at least the dimensions of the system, and one is probably going to be forced to set up the shock-producing mechanism in a little more detail; one has to keep in mind that in dealing with ordinary shock-waves one is lucky indeed that one can treat them completely locally, remote from their origin and any end conditions. This is only true because the equations of motion in aerodynamics have the nice feature of having these narrow zones like boundary-layers and shock-waves which couple one equilibrium state to another equilibrium state. This is by no means true for a shock-wave, or what should become a shock-wave, in a very viscous gas: a piston moving into a very viscous gas takes some time to produce a shock-wave, and consequently if something else interferes no shock may be formed at all. Whether a collision-free shock exists is, of course, intimately tied up with the possibility of defining an entropy function of state. In passing through a shock from one equilibrium state to another one cannot satisfy conservation of momentum, energy, and mass without increasing the entropy; hence one must be able to define an entropy in a complete shock theory. I feel that this ultimately should not be a «sort» of entropy, but it should be an entropy that can be connected up with the entropy of thermodynamics. If this is not done, I am sure someone will be able to produce a cyclic process with greater than Carnot efficiency. These are questions which I am quite sure can be settled, and the necessary experiments can be done within a limited amount of time. They are not very large-scale plasma experiments—one is not trying for fusion. But we will surely find many very interesting phenomena regardless of whether the collision-free shock turns out to be a useful element in astrophysical discussion



or not. In the discussion on these shocks there is one outstanding experimental result apparent: in the course of these four symposia *i.e.* in the last 12 years, one has reached a point where practically every figure in the variables of state that has been quoted in this symposium is not completely out of reach of laboratory experiments. Laboratory experiments on gas motion in the solar corona with its temperature of some  $10^6$  °K would have looked perfectly ridiculous in Paris in 1949; today the temperatures are within reach. In general the possibility of investigation of astronomical or astrophysical phenomena in the laboratory has increased enormously. Furthermore the interplay between shock-waves and turbulence, and its relation to astrophysics, is becoming closer. This is particularly evident from Petschek's discussion. Unfortunately, we cannot produce in the laboratory the size and the gravitational fields; this is something which I think even most of us today would consider as necessarily left to the astrophysicists proper. Even in the prevailing satellite craze, I do not think anyone yet thinks of building a satellite big enough to show there significant effects.

— R. N. THOMAS:

Let me turn to look at the Symposium from the standpoint of the astronomer, in terms of the background that LIEPMANN laid. Really there are two viewpoints that must be considered. From the standpoint of an astronomer anxious to find a ready-made analytical approach; what kind of structures do there exist in aerodynamics, relative to the problems found in astrophysics, that we can take over, use and apply? In essence, LIEPMANN has given a survey to answer just this viewpoint. Then the astronomer might ask, what is the viewpoint of the aerodynamicist? Why should he be interested in such things, other than as a kind of altruistic consultant? It would seem that the astrophysicist's hope of attracting the aerodynamicist lies in the possibility of enlarging the domain of the aerodynamicist's experience. Now LIEPMANN has just given a discouraging comment on this last—by stating that more and more we can do in the laboratory everything that the astronomer can do, with maybe the exception of gravitational fields. However, I would point out two other aspects. One is from the standpoint of time-scales; in astrophysics, one can get steady-state phenomena departing rather widely from local thermodynamic equilibrium; and he can do this at quite low densities, so that collisions do not predominate everywhere as the important rate process. Second, and correlated, radiative phenomena have a much greater importance in the astrophysical situations: the coupling between velocity and radiative fields in determining the thermodynamic state of the medium becomes very important. (There is, of course, a third aspect, very large dimension in the astrophysical case, which is of importance both in hydromagnetic and in radiation problems. I want here, however, to emphasize the other two points.) So maybe these

aspects offer an interesting extension of the region of aerodynamical experience, sufficient to characterize the astrophysical domain as having unique properties.

Now, if I want to look at these problems from the standpoint of both the astronomer and the aerodynamicist, it is necessary to keep in mind that both somehow have to get in the mood of the other's viewpoint. From the standpoint of the astronomer, he must somehow buy the aerodynamic terminology and approach. For example, for years we have been talking about «astrophysical turbulence», and have been asking is it a question of semantics, whether or not we agree that this is a good terminology, or is there something misleading in buying such an «expropriated» term. I suggest that when the proceedings of this symposium come out, the astronomer reads again the comments that CLAUSER and LIEPMANN have made on this point, and searches, his soul a bit. This example seems to me something very worthwhile taking to heart. If we could somehow get into the habit of thinking on the basis of existing background, then one has a better chance of using the aerodynamic concepts to solve astronomical problems.

From the standpoint of the aerodynamicists, I would pick up the point that LIEPMANN made a few minutes ago, that it would be nice if the astronomers would give him a table of velocities, temperatures, and so on. I would just point out one thing—that to get these quantities that would be put into a table, one requires oftentimes almost to solve just those problems which one asks for the help of the aerodynamicists on and for which they ask such a table as a starting point. Two examples on this: one, the point that SEATON raised yesterday—with respect to the corona. If one says there exists an electron temperature, and a kinetic temperature, which are different by a factor of two, and asks for interpretation of this, then one wants to be sure that his interpretation is precisely what he says. As SEATON pointed out, there were essentially three, possibly more, alternative explanations on this: — One, we just have wrong collision cross-sections, and so deduce the wrong electron temperatures. Thus, a table pointing out that there exists a difference in electron temperature and kinetic temperature is already an assumption. Second, maybe this difference really exists. Third, maybe we have a «turbulence»; and fourth, maybe we have a non-Maxwellian velocity distribution. Now all of these things possibly are what should go under the column as uncertainties. But you see the uncertainties reflect what it is that I am talking about. I have to discuss each of the alternatives from the standpoint of asking their likelihood.

The second point which I think is something very worthwhile keeping in mind is when you say that you are impressed very much by the sun as a source of information. A few years ago some astronomers would have said we need Mach 3 turbulence in the solar chromosphere to explain the observations of the change in intensity of the spectral emission as a function of height, which

was interpreted as the density gradient in the atmosphere. Astronomers would have furnished you tables of density, temperature, composition as a function of height, and asked an interpretation of how this Mach 3 turbulence exists. Now, applying some ideas of non-equilibrium thermodynamics, we come up with a quite different structure of the same atmospheric region. It seems to satisfy hydrostatic equilibrium. The aerodynamic problem seems to be, what kind of a non-radiative energy source that provides negligible momentum transfer can exist? And we give you a quite different table of densities and temperatures. The aerodynamic problem, as it existed a few years back, was a question of a momentum supply, with no energetic coupling to the ambient temperature; the current problem, a source with strong energetic coupling, but no momentum supply.

This emphasis on looking carefully at the ultimate basis for the kind of astronomical information you want, is what PECKER and I tried to stress in the opening summary paper—which essentially fell flat. The point is, we observe spectral line-profiles—that's your table of directly observed quantities; temperatures, densities, velocities—those are inferred, their values are a function of the inferential procedure, which often already has in it an assumed aerodynamic solution, at least conceptually. An example is the existence of Mach 3 turbulence that doesn't couple energetically with the ambient atmosphere. In certain simple cases, we can get information from line-shifts alone, and do not require in a first approximation the analysis of the profile; but these are exceptional cases.

With these cautions in mind, let me summarize what it seems to me the aerodynamicists have said about the problems the astronomers have posed, and add a few comments from the above directions. LIEPMANN has broken down the problems which stood out for him simply as problems; let me recast the approach slightly, in terms of the astronomical material upon which the problem rests.

*The question of non-thermal velocity fields that are described as random at a particular point in the atmosphere.* — The material discussed mainly refers to a small-scale motion, and the results can be broken down into those coming from total absorption in the line, and those coming from an analysis of line-profiles. These velocities are what the astronomers call «microturbulence».

Based on Anne Underhill's summary of results from total absorption it seems to me the aerodynamicists say the following: «We would indeed be very much surprised if you did not find existing in the stellar atmosphere small-scale motions, whose velocities are smaller than the local sound velocity. It appears that your results do indeed give such subsonic velocities, so we find nothing surprising. It is not clear to us from your methodology that what you call «microturbulence» is wholly a velocity, rather than some



neglected effect in your analysis; but since your derived values are upper limits on any velocity, we find no cause for alarm. » In addition to the remarks PECKER and I already presented, I would only add that I personally still find it difficult to see why compressibility and dissipation effects are obviously so negligible as a number of the astronomers seem to believe; for these « microturbulent » velocities extend to  $M \sim \frac{1}{3}$ —nearly 1. Schatzman's talk in Part III-B emphasizes this same remark.

If, now, we turn to material from analysis of line-profiles, then it seems to me that in this symposium we have talked surprisingly little about many of the problems which astronomers have discussed so extensively in the literature and which are necessary background for any aerodynamic synthesis: the questions of the depth-dependence of the small-scale motions, and their possible anisotropy. These points have been mentioned in passing, several times; but nothing like the desired compendium of knowledge has been presented for the aerodynamicists to see, and possibly find problems in.

Thus, all that has really been said about these small-scale random motions is that their magnitude is not surprising; but nothing else, because of lack of presented material. I question whether this underemphasis really represents the astronomical situation relative to material or to interest — or does it?

We find an even greater absence of discussion of material relating to large-scale random motions; indeed, relating to large-scale motions of any sort, except systematic motions whose properties exhibited here rest mainly on line-shifts, or correlated properties of the (optical) continuum. This underlies my remark that all the rather elaborate considerations PECKER and I put out in the introductory paper were really rather beside the point, as regards the eventual emphasis of the symposium as it has evolved. I do not mean to imply that we in any sense repeal the paper; PECKER comments that he thinks it will be a good paper in 1974. That is, by then we may, at one of these symposia, be basing our conclusions upon material derived from line-profiles analysed according to the methodology critically discussed there. I am sure the aerodynamicists were aware of the interchange among the several groups of astronomers, as to how elaborate a methodology is necessary for use in discussing current astrophysical data on spectral line-profiles. I would simply like to stress that getting such material, and analysing it, is really the prerequisite for that detailed picture of the aerodynamics of a stellar atmosphere which we all desire. Turn then to the problems which have come up from the more restricted kind of data, which essentially deals with systematic velocity fields.

*Systematic large-scale fields.* — Again, let me categorize these slightly differently than did LIEPMANN, emphasizing different aspects. Three problems stand out: A) the « orifice » problem, or continuous mass flow from the star regarded as the diverging nozzle problem in aerodynamics; B) two



sets of problems connected with the He-He ionization zones; *C*) the problem of propagation of a compression wave in a region of decreasing density.

Problem *A*) has already been discussed, and summarized. I would only comment on this from the standpoint of extending the range of aerodynamical experience — namely, the one thing not covered by simply applying the classic aerodynamic literature to this is the question of how do we take into account the radiation field, so far as excitation of electronic degrees of freedom, the energy leak, and an energy dissipation term are concerned. This essentially enters both through the specific heat ratio,  $\gamma$ , and through problems of internal excitation, which again relate to  $\gamma$ . While maybe I can solve the problem by talking simply about a range of  $\gamma$ -values, very probably the way that I have to go at it, is to start talking about coupling to the velocity field — and studying the solution as a function of position rather than the solution simply as a function of a constant  $\gamma$ -value.

Problem *B*), the He-He ionization zone, has really two sets of problems associated with it: the ionization zone as a source of convective motion for all stars having such a zone, and the ionization zone as the combined thermal valve giving a phase change in, and as an energy source maintaining, the pulsation in the pulsating variable stars. The first point is of especial interest with respect to the preparation of a list of observed phenomena for the aerodynamicist. For, if I abstract correctly the discussion centered on the presentation by Mrs. BÖHM-VITENSE, SPIEGEL, and MALKUS — if presently existing theories hold at all, they hold only up to a point significantly below where we have direct observational material bearing on the problem. The quasi-mixing length theory gives reasonable results, if at all, only up to optical depth (in the continuum) about 1, thus below the region observed in spectral lines. In the transition zone, where we have some possibilities of data — observations of weak lines, center-limb contrast of granulation—the theory cannot be expected to give good results. Questions of compressibility certainly enter here—recalling the discussion of the other day, I would note that the Mach number becomes about  $\frac{1}{3}$  at the beginning of the transition zone, so compressibility cannot be neglected. Thus, the questions of the distinction between random noise, and eddy-type turbulence, which have been touched on several times during the symposium and by LIEPMANN a few minutes ago, enter. To my mind, the approach started a few years ago by MOYAL and UBEROI still offers the best direction to start getting some kind of a picture.

The second problem on the ionization zone, connected with the pulsation, touches both on the solution to the pulsation problem in the interior, where we do not have direct observations, and in the atmosphere, where we do. If I look at the situation in terms of the piston problem, which WHITNEY has summarized, then I personally still prefer to attack the general problem as two coupled problems. One, is the solution of the interior problem, with,

if Ledoux's suggestion is correct, the ionization zone fixing the energy source maintaining the pulsation. The solution to this problem provides the initial value conditions for treating the piston problem—viz. provides amplitude and phase relation between pressure and radiation flux. Then, a study — as WHITNEY has summarized — of the compression waves moving outward into the atmosphere provides both the energy dissipation in the upper atmosphere and the observational material for checking the theory. Again, it is the radiative flux term that provides the unique astrophysical element, over the straight aerodynamic solution of a compression wave in an atmosphere of decreasing density. Then one must, of course, return to the solution in the interior, to make sure the energy supply from the ionization zone does indeed match the energy dissipation computed for the running wave, so it is an iterative process.

It is not clear to me which aspect of this solution LIEPMANN referred to, when he said he looked for great progress within the next four years; it seems to me it was the problem of the interior. But let me stress that we have a detailed observational test only for the part referring to the atmosphere. And, there, we must begin to look into the problem of the detailed interpretation of a line-profile in an atmosphere with sizeable systematic velocity fields, which couple with the thermodynamic state of the atmosphere, both directly and through the radiation field. So, we require two kinds of work — on the aerodynamical problem and on the analytical problem of interpreting spectral line-profiles — they are, of course, coupled. It will be interesting to see which is the closer, Liepmann's estimate of four years on part of the aerodynamic problem, or Pecker's of 1974 on the interpretive problem on line-profiles.

Finally, problem C), that of a compression wave in an atmosphere of decreasing density, grades directly into the second problem of B). Indeed, there are two problems—an individual wave propagating outward, and a statistical array of waves. The former, when it embraces the whole atmosphere, is just the cepheid problem discussed. The other is the ensemble of waves, a brief approach to which was outlined by WHITNEY and KROOK, and which LIEPMANN thought offered a good hope of rapid extension of present ideas. I would like only to elaborate on one point raised by LIEPMANN. He emphasized that it would be very nice to treat this problem as a whole, to begin with the ionization zone, solve the convection problem, produce the random noise waves, follow them up into the atmosphere and investigate their energy dissipation, thus fix the temperature distribution in the atmosphere. I would like to emphasize that one is led into another class of stability problems in such thinking. For, it is not sufficient to consider only the local mechanical dissipation of energy — one must ask how the energy gets away from the point of dissipation. While in the laboratory, this is largely by conduction: astrophysically, it is wholly by optical radiation except near very steep tem-

perature gradients. And when one investigates the outward increase in temperature accompanying such a process, he finds there exist very definite regions of stable radiative dissipation, together with essentially « jumps » in temperature, corresponding to radiative instability. Thus, one must couple to this problem posed by Liepmann a solution of the radiative transfer equation, indeed the solution under a non-thermodynamic-equilibrium kind of situation. This stresses again the point I think is the most important, in discussion of aerodynamical problems in astrophysics. Essentially, laboratory aerodynamics deals with mechanical transfer problems; the astrophysics of static stars, with radiative transfer problems. When one treats aerodynamic motions in an astrophysical environment, he must combine the radiative and mass transfer problems, and the coupling introduces many problems outside the experience of both aerodynamicist and astrophysicist. Our greatest need is to develop some sort of feel for this new range of problems, so that we are not too quick to simply take over a solution from aerodynamics to an astrophysical situation.

In this last sense, there falls very close to the random noise problem another class of problems, just touched on briefly, that of the ejection of material into the stellar atmospheres in « jets ». The spicules form one example; possibly some of the eruptive prominences form another. The problem has hardly been mentioned in the symposium, so should not be dwelt on at length. I would only mention it as a possibly simple example of a place to study the coupling between radiation and velocity field just mentioned, from stability considerations. One might look into the problem of a supersonic jet, where ambient conditions correspond to high enough energy that radiative loss becomes a significant problem during the compression phase of the jet motion. This might provide a place to develop some kind of physical feeling for the difference between the wholly aerodynamic treatment of a well-known problem, and the astrophysical perturbation.

Thus, to me, in maybe an oversimplified way, two kinds of problems stand out. First, how to extend the range of astrophysical information that may either inspire or check an aerodynamic theoretical approach—and here we deal with the subject of sophisticated interpretation of line profiles. Second, how to develop our feeling for the change in aerodynamic solution coming from the introduction of a significant radiative energy loss — or the methodology of studying coupling between velocity and radiation fields.

— R. LÜST:

I feel somewhat in the situation that I have attended a very nice party in the evening, then I am sent a guest book and have to write in something very nice. But already some people before me have done the same. Of course, this summary will be a very subjective one, since probably everybody likes to pick the points in which he was most interested.



I think it was really quite good for this present meeting that the magnetic-hydrodynamic phenomena were not brought into the discussion too much, since I think this is still the field where our knowledge is highly limited. But on the other side. I think the groundwork is laid for the direction of co-operation between the aerodynamicists and astronomers that really already exists in other than in this field. We may hope that in the near future we can enlarge it quite a bit. Then I would like to remark on the problem of the corona. Since now in the laboratories we have a good possibility to simulate the main features of the corona: for instance, radiation process and things like this, I think that it is really necessary that we put specific questions to the experimental physicists.

I would like just to summarize a few points that I think we have made some progress on. The first one was already mentioned—I think this was the mass-loss problem or the out-flow problem from stars. I do not refer to the sun, but to the stars in general; here new observational data have been presented by DEUTSCH on M giants — we can see there that circumstellar envelopes imply large outflow in the outward direction. On the other side, there are certainly quite a number of other stars where one knows that a mass flow is occurring, but these stars had not been really discussed in great detail in this meeting. For instance, I would just like to mention the B stars, which played a large role in the Cambridge meeting in England; for instance, expansion of the H II regions and things like this which are certainly connected to the mass-loss, but this point has not been discussed here. I think it was already mentioned by Liepmann that after some discussions there was some kind of agreement that there is now at least a first approximation to a good hydrodynamical model; namely, that we can describe the outflow by the hydrodynamical outflow in gravitational fields, and this is a very essential contribution from the aerodynamicist.

I would emphasize two aspect. Namely, first, that shock-waves might be really important for describing or fixing the boundary problems. I think this was the first heavy discussion between DEUTSCH and PARKER — what are the right boundary conditions? Solutions did come from the aerodynamicists, that we have to take into account eventually the shock-waves. And the second point was the very clear presentation of this picture by GERMAIN, who pointed out that we have just a one-parameter family of solutions, not a 2-parameter family.

The next important point in connection with the mass-loss problem, and I think this was also a step forward, that we could reach some kind of agreement in the solar wind picture; namely, that for the sun we should have hydrodynamic outflow, and that this should give us the so-called solar wind or the corpuscular stream in the solar neighborhood. If one compares the situation with ten years ago, there just the opposite assumption had been made for all these kinds of problems; that we have inflow. Really the same equations have



been used, as MCCREA pointed out; but now I think we have more and more evidence that we have really an outflow. There were really two points — first, that we got this description, and then that we could reach an agreement on the understanding of what should be the density for this solar wind in the neighborhood of the earth's orbit. On the latter, the evidence came from three points. First, for the first time there were some indications from the satellite observations on the density in the solar wind. Then secondly, from the comet observations. Third, from the geomagnetic observations in the polar regions of the earth. These three kinds of evidence point to a density somewhat smaller than  $10^3$  particles per  $\text{cm}^3$ . The average velocity for the undisturbed wind is about 500 km/s. But the important question which has been raised in this connection — and I think it is also true for the other stars, especially noting the observations by DEUTSCH for the M giants — is what really is the region around the star which is one way or another more or less still connected to the star? How far does this region go into the interstellar space? For this question it is important to see how one may have to place the shock-wave, if one is just treating the problem on a wholly hydrodynamical basis. For this some figures have been given, depending on the pressure in the interstellar space. But I wonder if we would get in this way the right answer, since probably some other kind of boundary conditions might be even more important; and also since these estimates which have been presented here have been based on a steady stream of outflow of corpuscles leaving the sun or the other stars. Especially, I would think for the sun it might be really important to discuss the feature of steadiness, even while taking into account the magnetic field.

Also I should mention one other point which has been discussed already by LIEPMANN. In the problem of outflow, a point which has not been really discussed is the rotation of stars, which might play an important role, and in this connection also the magnetic fields of the star itself. PARKER touched this point very shortly. It did not come into the discussion, what the best picture would be if the magnetic field would be really carried away to a large distance and twisted. Should one see it at a large distance — for instance, in the earth's orbit, or not? This would raise the possibility that the outflow starts at larger distances than have been discussed until now. These kinds of problems have not been discussed, and I think one should keep them in mind. One should note that already some observations on the interplanetary field have been mentioned, and the situation seems to be very difficult if one compares these first observations with the theoretical picture which he might expect for a solar wind. However, it is probably too early to really draw definite conclusions.

Now this brings us to the question, what is really the energy source for this solar wind. What is the temperature in the outer region of the star. Especially for the sun, what is the temperature in the solar corona? We heard

yesterday that there is disagreement between the electron temperature and the ion temperature. Until now it is not clear if there really is a difference in the electron temperature compared to the ion temperature — this would be one possibility, as has been mentioned by SEATON; but I think this question really depends very much on the exact computation of the relaxation time. The second possibility would be that there would be large motions in the corona. Finally it was mentioned that maybe they have not used temperature at all, and this then leads us directly again to the solar wind or the outflow of matter. Now I think it was clear from the picture of the hydrodynamical outflow that one needs quite high temperatures in this outer solar region to get such an extremely high outflow, and therefore the question has been raised, where is really the source responsible for heating up this outer region? There the idea is that this might be the hydrogen convection zone, and we have discussed several possibilities for heating up the chromosphere and the corona. Here I would like also to mention, as has been pointed out by Mrs. BÖHM-VITENSE, that we cannot expect in every star a hydrogen convection zone. For other stars — for B stars on the Main Sequence — one should not expect to have a hydrogen convection zone — only a very shallow one for the giants and super-giants. But on the other side, from the observational evidence, we got the impression that these hot stars, especially giants and super-giants, exhibit higher velocities, and therefore the question is in which way we can explain this kind of high velocities in these kinds of stars. There I think no reasonable answer has been yet found. It was mentioned that the rotation in these kinds of stars may be responsible for this kind of thing, but I think it is too early in the thinking to be sure.

Now going down into the solar hydrogen convection zone, it was explained that what the astronomers are doing is completely wrong and one cannot trust it at all — especially in using the mixing-length theory. But I think we did reach some kind of agreement, that the theory was not so bad, to a factor 2-5. If one does not worry about a factor like this, then this kind of mixing-length theory is probably good enough. Of course, it was recognized that some work has been done to improve these theories on the hydrogen convection zone by really looking into the more detailed problem — discussing and treating the stability problem — this has been done by the Princeton group and by BÖHM. There we learned also one other important thing from the studies on the hydrogen convection zone, and this is that one may expect that the temperature fluctuations are not correlated over the whole wave-number scale; that there might be a correlation only for a small wave-number scale, but not for the larger wave numbers. So this might play a role for the interpretation of observations. This brings me now to the observations; where we really learned quite a number of new details, especially due to the improvement in space resolution, from the balloon pictures, and from the pictures

by LEIGHTON. May I just mention one number which looked to me somewhat new in these observations, and this is the lifetime of the granules. It is somewhat larger now, due to the new observations, than one has assumed earlier from the old observations. Also the question had been raised if one could really speak of a well-defined lifetime for the granules at all.

Then further, I would like to mention one additional point which was completely new, observations by LEIGHTON that one has some kind of oscillations in the velocity fields with period of about 5 minutes. Interpreted correctly, this might be of importance. This brings me back to one point which has been mentioned already by LIEPMANN, when he spoke about the cepheids and the mechanism of the cepheids. He mentioned that probably the mechanism for the cepheids is such that positive and negative damping just cancelled, and he mentioned also the possibility that one has random kinds of excitation. It might be that this is the mechanism responsible for this kind of oscillation, since we have this random noise at the top of the hydrogen convection zone, and the observed period for this oscillation is very near to the frequency of the solar atmosphere.

Now I do not want to say too much about the active sun. This field has been touched only just at the beginning, and due to new observations on the magnetic fields, we have now a better detailed picture of the magnetic field. For these larger regions, the magnetic field strength is on the order of 50 gauss, and not as one could see it on the Babcock diagrams, only of one gauss. This refers to the calcium regions. Also, we had some discussions on the motions within sunspots. Here it was good to learn that apparently the observations are becoming better and better, so that probably in the next few years there might be some hope to decide this question, if the motion is really along the field or if there is some discrepancy, as there would be with some kind of motion across the field.

I would like to mention very shortly the problem on the collision-free shock-waves. I think this problem really only at this stage permits comment from two sides. Namely, that one has to do experiments and also has to try to get a real theory. But I think that also for the sun this kind of problem is really of importance.

Finally, in discussing the problem of generating waves by the hydrogen convection zone—we discussed just the propagation of these waves, but we did not really discuss in too much detail the dissipation phenomena in the solar atmosphere. I think this is still an important problem, where much work has been done, since probably the kind of dissipation mechanism which has been discussed until now has been just dissipation by shock-waves, and also some dissipation by ambi-polar diffusion. The possibilities of dissipation mechanism will be probably much larger if one is really studying the plasma in more detail. Since we know already from experiments that we have many



more instabilities than we first thought of—the plasma always behaves more or less unstably—I would think that this kind of phenomena, which one is just at the beginning of understanding, might have played quite an essential role in understanding phenomena on the active sun. Also, there the point which was mentioned by PETSCHKE—the question of conductivity, a reduction of conductivity might be really quite important. Until now I think we have always been simplifying the problem to the extreme, by assuming infinite conductivity, and I believe it is fair to say that probably all the problems on the active sun—especially flares and other things—can only really be understood if one takes into account finite conductivity and tries not to make this very simple assumption of infinite conductivity. But if one then really wants to take into account finite conductivity, I think it is really essential to understand what is the mechanism for the conductivity, and what is the value for the conductivity, which one should take into account.

### *Discussion:*

#### — A. UNDERHILL:

THOMAS has noted that at this conference little discussion has centered around information about velocities that may be determined from line-profiles. This is clearly because of the difficulties of separating the true physical information from the assumptions involved in the interpretative processes. Thus, I think astrophysicists must continue to examine the unfolding processes by which they derive information. They must develop methods which determine the physical results in a manner in which we may have confidence. As a result of this conference I feel that we may be able to make some progress in this difficult field. For some time, not too much work has been going on in this field, perhaps because people did not realize its necessity. Furthermore, I believe that if now at this very moment we iterated the conference, and started again, that a better appreciation of the meaning of the results presented, particularly the observational results which were presented in the first few days, would result.

#### — A. J. DEUTSCH:

I should like to say that as soon as I get back to Pasadena I feel inspired to press forward a program of observing the outer envelopes of stars. Largely as a result of our deliberations here over the past week, I feel that, with the collaboration of the aerodynamicists, the time is now right for us to investigate those appendages of the stars that must be analogous to the solar chromosphere and the solar corona. These heretofore have received very little attention. There have, of course, been good reasons for this; these objects are not



easy to observe in integrated light. But now I think we see our way clear to specific observational tests for investigating what stars do indeed have extended atmospheres of this kind, and to discover in what respects they resemble, and in what respects they differ, from the familiar example of the sun.

Several other speakers have referred earlier to the fact that our operations upon stars are relatively coarse. We have to use the means at our disposal, and unhappily they are pretty gross. So that we shall never, for example, be able to get the beautiful, detailed information about stratification that the solar observers have extracted from years of patient research; or about the very complicated and fascinating velocity fields which we have heard discussed, actually starting down in the layers of the sun which we cannot see, penetrating into the photosphere, and extending right into the reversing layer where the absorption lines are formed, the chromosphere, and the corona. We never, in the foreseeable future, will be able to get detailed information of this kind about the stars. And therefore I feel it will be necessary for us to exploit very fully those observations that we can obtain by more or less conventional means. In addition, we must certainly apply the new techniques of space and satellite spectroscopy, whenever these become available, for helping us to acquire more data about the radiation in the vacuum ultraviolet, which arises in stellar chromospheres and coronas. I think that this opportunity may not be too far off. Third, we must be guided by theoretical considerations suggested to us by the aerodynamicists. And fourth, we shall have to lean heavily upon analogies drawn with the sun.... I suspect that, as a result of the last couple of weeks discussion here, many of you share my feeling that the sun is in very good hands indeed, and that before very much longer we may hope to have a quite complete and detailed picture of the brightness and velocity fields in the accessible layers of the sun. This should act as a very secure guide to the theoretical people, and particularly the aerodynamicists, in formulating the physical laws that govern the outer structures of the sun. Perhaps we will also gain enough insight into these matters that then, with really much less detailed information to go on, we can apply the same kinds of arguments to the stars.

We shall have to proceed cautiously in any case. You remember that there was a table on this blackboard a few days ago, where we listed the relevant parameters of the flows that we observe in the expanding atmospheres of stars. These parameters are the thermal velocities, the escape velocity, the flow velocity, and the density. We found that these parameters range over several orders of magnitude; and that not only do the parameters themselves span several orders of magnitude, but that their ratios also span several orders of magnitude. Apparently nature is able to build the outer envelope of a star according to any of a number of different plans; and I suspect that in the case of the stars it will be necessary for us to lean rather heavily upon the kind of

theoretical considerations that you people have suggested in order to decide which of all possible plans nature has decided upon. Even our vocabulary seems to be rather inadequate for the job. I am a little reluctant to speak of the corona of an M giant when most of the spectrum lines I observe arising from this structure are lines from Fe I. I cannot see any lines with excitation potentials above a few tenths of a volt; in some cases I cannot even see anything above .01 V. Maybe it is a corona; but it is a very different kind of structure from the sun's corona, upon which we have concentrated much of our attention. It is quite clear that in extending the theoretical arguments that are immediately suggested to us by the solar corona, we want to keep in mind the possibility that in different stars the relevant parameters can span a very wide range indeed.

There were a few problems which were put forward in connection with steady outflow from a star, which were discussed only very briefly because of the limitations of time. Perhaps it would be appropriate to mention these again. First of all, there is the question of the appropriate boundary conditions to use in discussing such flows. Arguments have been given to show that the solar corona, for example, at a temperature of one or two millions of degrees, cannot be in hydrostatic equilibrium with the interstellar medium and that it must therefore expand. If one deduces the consequences of this, he finds that it must expand with a speed of the order 500 km/s, in apparent agreement with some observations. But is it obvious that the temperature is the independent variable, that it must be a million degrees, and that the velocity must accommodate itself to satisfy this condition at the inner boundary, while the pressure goes to the interstellar value at the outer boundary? I do not know, but certainly there are alternative formulations. We require a general physical discussion of the way in which it is appropriate to tie together the near and far boundary conditions. The latter may be specified as the terminal velocity, and the terminal pressure, or the temperature and the density separately. The former must no doubt be the fluxes of energy, mass, and momentum coming through the photosphere of the star. Having specified these two end states, can we then establish that there is a unique path the gas will follow? The aerodynamicists have given me some reason to believe that indeed there is and that these problems can be solved.

My colleagues in observational spectroscopy, and I, will have to do a lot of careful work to establish whether the consequences of theories of this kind are compatible with the observations. There has seemed to be general agreement that we can understand the flows at best only by supposing that there is always a high temperature region closely surrounding the reversing layers of the stars. There is some residual doubt whether in fact there is always room inside the shell we observe for an envelope of this kind, which would remain virtually invisible. The question has to be investigated in a quanti-

tative way. There is also a little more information we may get about the geometry of the structure by considering the binary systems, where we have not one but two lines of sight through the flow. And there will be fascinating questions, which were referred too briefly, having to do with other possible observational consequences of the existence of extremely large shock fronts around stars. What is the nature of the dissipative process in the shock front? Is it possible that this process has anything to do with the generation of non-thermal radio noise? What is its importance in the modulation of cosmic rays in the neighborhood of the sun, and in establishing the nature of the cosmic radiation throughout the galaxy? What happens, if anything, when these shocks collide with each other? For, if stars are to have enveloping shock fronts around them with diameters of some hundreds or thousands of astronomical units, space then is not so terribly empty any more, and at any one time in the galaxy there will be some collisions occurring. Does this lead to observational consequences? No doubt we shall have to look rather carefully into the stability of whatever kinds of flows we arrive at by a straightforward solution of the momentum equation; are all such flows stable against mechanical and thermal perturbations?

I wonder whether it will be possible for us to extend considerations of this kind so that we can make progress in discussing some of the more exotic kinds of stars that were not mentioned here at all, or only very briefly in passing. These are clearly suffering the same kind of fate as normal stars, but according to a much more elaborate pattern. There are stars, you know, in which we see not just the ordinary set of lines produced in the reversing layers, and the displaced set of zero-volt lines produced in an expanding shell; but in which we may see—and this is no exaggeration—up to 6 or 8 sharply defined components of the same line. Each has its own radial velocity, which remains virtually constant over a period of years. What can the hydrodynamics of this process be? We must be looking at a series of shells, one within the other, shells that are remarkably stable. How does it happen that we observe the atoms which have one velocity, which presumably lie in one shell; and the atoms which have another velocity, which lie in another shell; but nothing in between? It is quite clear that this year we do not observe in a shell the same atoms that we observed last year. They have moved out to some place else; we are observing a level in the gas. Why is it that we cannot see the atoms between the levels?

What about all the problems having to do with catastrophic mass-loss? How about the nova phenomenon, which we agreed to refer to only very casually here? Or the phenomenon which produces the planetary nebulae? You remember that the problem was posed as to what would happen if, in an infinite layer of uniform gas, one were to set impulsively in motion a certain slab of gas and ask for the subsequent motion of the gas both in front



of the slab and behind the slab. In particular it would be fascinating to know whether, and in what circumstances, the consequence of such an impulsive disturbance will be to detach a slab of gas from the rest of the gas. Or, if not literally this, then at least to produce a very deep density minimum. This might well correspond to the kind of thing that we have in the planetary nebulae, or in the novae.

There are some of the problems that we did not have time to discuss.

Finally, I want to say what very good news it is that LIEPMANN and, presumably, some of the other aerodynamicists are now, I think, more interested than ever in attempting to simulate astronomical phenomena in the laboratory. He did say that he thought it would be some time before he could incorporate an arbitrary gravitational field in his experiments. On the other hand, I think it was he who, at the beginning of his talk, indicated that the effect of the gravitational field—at least in the problem of spherical outflow—is just that of a nozzle. So possibly with a sufficiently clever geometry in his experiments, he will find that he can simulate the gravitation field too. It seems to me that, with so many avenues of exploration open to us, in the course of the next few years, we may confidently expect some pretty exciting advances in the study of aerodynamic phenomena in stellar atmospheres.

— B. E. J. PAGEL:

I would remark on the widths of the lines in the corona. In discussing the discrepancy there between the electron temperature and the kinetic temperature—it was mentioned that one possible explanation was macroscopic motions. I would just like to show that a macroscopic motion necessary to produce this effect seems to be perfectly compatible with figures given by PARKER in the theory of the solar wind. This might be a means of putting the two effects together. If you consider the limb of the sun, and a region of the corona where you see through a few scale heights—actually you see quite a distance because the densities do not vary rapidly along the line of sight—contributions to the observed intensity will come from a horizontal distance equal to an appreciable fraction of the solar radius. So, if material streams outwards at say 50 km/s, then one will see a component of velocity dispersion along the line of sight of the order of perhaps one-half of this, or perhaps a little less—say 20 km/s.

Now the thermal velocity of iron atoms at a million degrees is roughly 15 km/s, so it is clear that this velocity dispersion is of the same order as the thermal velocity, and is enough to raise the temperature that you would deduce from the line-widths to 2 or  $2\frac{1}{2}$  million degrees.

— G. K. BATCHELOR:

I have a few remarks which are not actually summarizing but are personal comments on what I think is worth remembering in the conference.



I think that as in the case of the three preceding meetings, the fluid-mechanics people have given the astrophysicists the distinct and correct impression that we know little about turbulence. Practically no speaker in the sessions was able to say anything about turbulence at all, without somebody else questioning his remarks, or saying, well, it may apply somewhere else but it does not apply here, or adding that some more qualifications have to be made. Turbulence is a messy subject, which intrinsically does not allow very precise and specific statements, and it is also a subject about which we know very little by comparison with, say, gas dynamics or magnetohydrodynamics; in fact, we know practically nothing. Two particular aspects of the turbulence problem were raised in the discussions of the last four days which I think are interesting. One is the aspect touched by LIEPMANN in his summary; namely, the concept of fairly definite patterns of motion in what we usually call fully developed turbulence. It may not be true of all steady fields of turbulence that these ordered patterns exist, but it is certainly true of some. Thermal turbulence is probably one of them; likewise turbulence generated in the space between rotating cylinders, and probably jets and wakes—in these cases one can recognize the existence of certain large-scale structures which qualitatively resemble what one would find from stability analysis for the mean velocity profile which exists in the turbulent flow. Now I do not think the concept of stability of fully developed turbulent flow is yet wholly precise. What we have to think about—I suggest—is the sense in which these ordered large-scale features represent some kind of finite-amplitude disturbance which exists in the statistically-steady turbulent state. It is natural to think of these ordered motions as representing one single mode which has been selected by a process of selective amplifications of the kind we know about in linear stability of laminar flows. But I don't think that notion by itself makes sense, at any rate not if taken in a simple or straightforward way, because it leads to the idea of a turbulent state with the energy partly distributed over a continuous wave-number spectrum and partly in a single spectral line representing the particular mode that has been selected by the instability. However, we believe that owing to the non-linear effects, which are undoubtedly important, the energy which is confined in a single line would very quickly be spread over a whole continuous range of wave numbers by the interaction with the continuous part of the spectrum. Possibly the position is that there is a peak in the spectrum rather than a single line, a peak whose width is determined by processes which are at this moment unclear to us. However, some of the information presented during the meeting about the granulation in the convection zone of the sun does force one, I think, to recognize this as a definite and interesting problem. When I first saw the photographs of the solar granulation published by SCHWARZSCHILD (*Ap. J.* 1954, 130, 345), I was struck by the apparent regularity of the cells—they look approximately like the Benard

cells, but yet they are existing at a Rayleigh number which is far above the critical value and are existing presumably under fully developed turbulent conditions. They are not perfectly regular, but they have considerable regularity and to understand how that regularity can exist in that situation is an interesting problem.

A second aspect of the turbulence problem which has not been mentioned today, but which did strike me as something to be thought about further, is the failure of the similarity theory to predict correctly the distribution of mean-square temperature fluctuations in thermal turbulence. Here, I am referring to thermal turbulence of the simplest kind, generated by a heated lower boundary, and with rigid boundaries. As MALKUS pointed out in his talk on Friday, such experiments as have been made suggest that the root-mean-square temperature fluctuation falls off with height as something like the reciprocal of the distance from the lower boundary. The simple similarity arguments which suppose that only the rate of transfer of heat across a horizontal boundary is relevant produce a variation of the mean-squared temperature fluctuation as height to the minus  $\frac{1}{3}$  power, and are thus in conflict with experiment. Malkus' theory is or is not right—the theory is difficult to understand and requires a lengthy discussion. But leaving that aside for the moment, it is interesting to ask why the similarity theory is not right. Malkus' suggestion is that it is incorrect because it takes no account of the effect of the viscosity and conduction in the region which is not near the lower rigid boundary. He may be right—it is hard to think of any other reasons why the similarity arguments should fail. If he is right, and if viscosity and conductivity are important in their effect in the interior of this flow, then that raises interesting questions and requires some revision of our thinking about turbulent flows of this kind. These are two aspects of the turbulence problem that I at any rate would like to carry away in my head for further thought.

Finally, if I try to characterize this meeting by a few words, it would be something like this. You remember that previous meetings have been summed up in a few words and I suppose it is always handy to have a label. The first one was largely devoted to turbulence and magnetic fields in the galaxy. The second, for the most part, to shock-waves and turbulence. The third to magneto-gas-dynamics. This one is perhaps not as easily summarized, so I would not suggest that the phrase that I am going to use is appropriate for everybody, but for me the problem thrown up most clearly is that of turbulent convection. Everyone will have his own particular label, but that is the one which seems to me to characterize this meeting in contrast to the three preceding ones.

— E. SCHATZMAN:

I think that we should add the problem of the influence of the radiation field in aerodynamics that has been stressed already by THOMAS. I think that

is very important to astrophysics, and it does not yet enter heavily in the laboratory experiment of aerodynamicists. When we introduce a radiation field into the equation of aerodynamics, we get into trouble with the equations because they become incredibly complicated. We have tried once or twice a linearized approach to the motions, including the radiation field and even then things are horribly complicated.

### Closing remarks to the meeting by M. Minnaert.

I should like to present a few general impressions from this interesting meeting of scientists working in two different fields. It has often been noticed that progress in science is made just on such meeting points, and I think this specially applies to astronomy in combination with other sciences. Astronomy in the last 50 years passed successively through the age of optics, then of atomic physics, then of nuclear physics, and now again of aerodynamics and electro-magnetism. At first sight I get the impression that the contribution of aerodynamics to the development of astrophysics has been less spectacular, less sudden perhaps than the progress made at the moment when Bohr's atomic theory was discovered and applied by САХА to stellar atmospheres; or at the moment when principles of nuclear physics were discovered, and fusion processes were found to be the main source of energy in cosmical bodies and the source of evolution. But this might well be a perspective effect. In the time when you are living, you always have the impression that things are going slowly; and when you are looking back, then you see that really in that period in a short time a considerable progress was achieved.

Such concepts as convection, shock-waves, magnetohydrodynamic waves are so fundamental now in astrophysics that we could not miss them. They are part of our vocabulary, they play such an important role that looking backwards, perhaps in 10 or 20 years, we may also have the impression that this was really considerable progress in a short time.

It is clear *a priori* that the contribution of aerodynamics to astronomy must be very important; for after all, with very few exceptions, our universe is composed of gases. It is an aero-universe, and more specifically it is not only an aero-universe but an aero-dynamic universe. Everywhere we find that there are motions in stellar and interstellar space, even in these layers of the sun and the stars where you would have thought that there was at least radiative equilibrium. We find that inside that same layer there is «micro-turbulence»; and in deeper layers there are motions of convection; and then there are the chromosphere and the corona with their shock-waves. Everywhere there are motions; the whole universe is really an aerodynamic universe. It is clear by adding to astronomy the knowledge which we can get from aero-



dynamical science, a considerable number of new possibilities arise for astrophysicists, of which we have explored only a small part—we are only just starting.

To aerodynamicists it must certainly be interesting to see how their work can be applied to cosmical problems. There are quite a number of new questions which have been put by astronomy and which must be stimulating in all directions for aerodynamicists. However, it will be necessary that astronomers try to put their problems in a simplified and clear form. The construction of good models is one of the important and difficult tasks of science, one of the greatest mean of progress. I think that we should also train our students in *making* models, and that we generally don't; we give them models, and they have to calculate what are the consequences. Let them learn to transform the complications of nature into a simple model, which afterwards may be treated by well-known mathematical methods. If we compare the application of aerodynamics to the study of interstellar space, and its application to the study of stellar atmospheres, we see that there is this important difference, that the application to interstellar space could hardly be tested by laboratory experiments. There, we are confronted with immense spaces and extremely low densities, which could not be imitated in any way, and the only way to progress is just theoretical calculation and argument. But in the case of stellar atmospheres, there is a great promise in the possibility of making laboratory experiments; this came out very clearly in many aspects during this colloquium and must incite aerodynamicists to giving their attention to the experimental solution of the problems with which we are concerned here. In the meeting of scientists of two different groups, there is always a certain difficulty to be overcome before one understands the language of the other one. One might hope that between aerodynamics and astrophysics, there would be a common language which they both understood—that is mathematics. But it appears that this does not really help so much, because the difficulties of common understanding are not in the mathematical process, but in the translation from the language of aerodynamics into mathematics or in the translation of astronomy into mathematics. It is there that the difficulties are. Once you have made the model and the translation, the mathematical machine operates and there we all understand each other.

It seems to me that in certain respects we have over-estimated each other's knowledge. We could have perhaps prepared the audience better for the general lectures. Astronomers should have shown more slides about solar phenomena and should have made things a little more picturesque. In a similar way there are basic concepts of aerodynamics which were given often after they had already been used extensively in the introductions. These are things which occur quite naturally and would have been difficult to avoid. But for future meetings let us try to introduce always the necessary preliminary con-



cepts, let us encourage people to ask for explanations, to look very clearly at the fundamentals and not to be satisfied with general terminology but to ask critically about the basic points.

It may look to astronomers that some of the aerodynamical theories are somewhat far from astrophysical applications. On the other hand it is clear that we must first give our full attention to such pieces of theory, which are, so to say, machines which can be applied to many different cases. We must understand how a certain number of these machines work to be able to put them together afterwards, and to construct a whole factory, giving the results which we need in order to explain our observations. From that point of view, astronomers should have the patience to listen, to understand, to become familiar with different parts of aerodynamical machinery, even if they do not see at once the possibility of application.

It is a disturbing circumstance that often in the discussion it becomes clear that the astronomical observations, on which we should be glad to rely, are not so certain as one would have hoped; and that on the other side, the aerodynamical theories, with which the astronomers were at first so happy, are not so well-established either. And that on both sides there are doubts and contradictions, and people thinking in another way. I can give no consolation in this respect except to say that fortunately the complaints are reciprocal. But I think we have to be resigned—this is the difficulty of science on the march, that there are inevitably a certain number of doubtful points which must be overcome by gradual progress.

It seems to me that after what we have heard, we shall feel the necessity of reading more of each other's work; and in that respect I should like from the point of view of astronomers to ask that in the reports of this symposium there should be given a certain number of good references to general aerodynamical books. Secondly, and perhaps more important, that also specified references should be mentioned by those having quoted papers of the literature in the course of their introductions or in the discussions.

I think that on both sides, we had sometimes the impression that the other group puts too much emphasis on details, which might seem irrelevant. There has been a discussion on the temperature of the corona, and I heard an aerodynamicist saying « Well, is it so important whether that is one or two million degrees? » One must understand that in a certain number of cases such an apparent detail is very important, because it may disclose fundamental questions concerning the structure of such a stellar layer. And also from the other side, astronomers sometimes had similar impressions. The mixing-length theory, for example, which gives very useful and practical results and is extensively used, was criticized by aerodynamicists because it is not quite accurate in all details. It is clear that it is not easy to say where precision becomes essential and where it is simply a nicety which does not matter. This has to



be left to the critical sense and appraisal of each group, and no general rules can be given. It is only clear that, where details are not essential and only a matter of discussion between specialists, they should be left out in colloquia like this.

I have heard the question being put: « Will it be in the future necessary for students of astronomy to take a course in aerodynamics? » I would be inclined to say: Yes, certainly, and I think in many universities our students are having already such a course. Unfortunately, such lectures always give too much on one side and too little on the other side. It is impossible to have a special course of aerodynamics made in such a fashion that it precisely applied to the needs of astronomers. That would not be right, even—they have to learn something about aerodynamical science in broad lines, and then they must be able for themselves to study more especially these parts which are important for astronomy. One may hope that sooner or later there will be written a textbook on aerodynamical astronomy or something like that—especially applying to astronomers, just as we have now already textbooks of cosmical electro-magnetism or about radio astronomy, especially written for the astronomical applications. But let me ask also the reciprocal question: « Will it in the future be necessary for students in aerodynamics to take a course in astronomy? » There I am afraid that I cannot say yes. But I hope that aerodynamicists shall insert in their lectures a certain number of examples taken from astronomy, which may be very impressive because always it is beautiful to see how theoretical physics can be applied to nature on a cosmical scale.

I am afraid that I have perhaps emphasized a little too much the difficulties of common understanding—on the other side there is no doubt that for the great part, we have been able to overcome these difficulties, and that very fine and interesting results have been reached in this colloquium. They have been explained and summarized by several people here. I only want to emphasize particularly the very beautiful observational results which have been presented and which are fully appreciated when we take in mind the immense amount of work of organization, of perseverance which is necessary before such data are obtained. We must also remember that a quantity of new information may be derived out of this material. On the other hand, it is gratifying that there has been not only a series of beautiful observational results and a series of aerodynamical theories—but that on many points the connection has been made; for that was the essential moment of this colloquium and its real purpose.

---

PROPRIETÀ LETTERARIA RISERVATA

---